A. Jamme

Miscellanées d'ancient arabe XVII

Washington
December 28, 1989

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Thüringer Universitäts-
und Landesbibliothek Jena
Zweigbibliothek
Altertumswissenschaften
Nebenstelle Löbdergraben 24 a

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Foreword

The present XVIIth volume of my Miscellanées... is devoted to the review of the following publication:

A. de Maigret ed., The Sabaean Archaeological Complex in the Wādī Yalā (Eastern Hawlān aṭ-Ṭiyāl, Yemen Arab Republic). A Preliminary Report (Istituto Italiano per il Medio ed Estremo Oriente. Centro studi e scavi archeologici; Reports and Memoirs, Volume XXI), Roma, 1988, in-4°, XVIII-59 pages, 36 figures, 56 plates with 96 photos, and 83 pages in Arabic.

By way of foreword, G. Gnoli opens the English section with a short laudatory summary of IsMEO's activities and goals as a prelude to its work in Yemen (pp. IX-XIII = Ar pp. 7-11). Follows then an introduction by A. de Maigret (pp. XV-XVII = Ar pp. 13-16) which precedes his major contribution on a survey of the antiquities of the Yalâ area (pp. 1-20 = Ar pp. 17-39), about 50 km southwest of Mârib and about 35 km northwest of Wâdî al-Jûbah. While G. Garbini's study of the Sabaean inscriptions from three contiguous places called al-ʿAqûl, al-Jafnat and Yalâ is found in pp. 21-40 (summarized in Ar pp. 77-78), M.A. al-Iryânî's parallel work is given in Ar pp. 41-75, with a short English summary (pp. 41-44) limited to the "the three compl tely new words" (p.41). The book closes with B. Marcolongo - A.M. Palmieri's presentation of the environmental situation of the Yalâ area (pp. 45-51 = Ar pp. 79-83) followed by A. Lentini's short appendix on a preliminary pollen analysis (pp. 52-53). The central section of the book contains the illustrations, viz. 62 photos in 34 and 1/2 pl (pl. 1-11a, 15-37 and 56) and 36 figures (including fig. 18-27 with the drawings of 10 sherds) for archaeology, and 34 photos in 21 and 1/2 pl. (pl. 11b-14 and 38-55) for epi graphy.

The following pages contain my response to Gnoli's invitation that the present volume "is, in fact, the first scientific work of vast commitment to be submitted to the scrutiny of scholars" (p. IX).

I - General remarks.

1 - The first two unexplained elements of the inventory siglum, "Y.85," stand for "Y[emen 19]85."

2 - The spelling and pronunciation of the name "al-Iryânî" (cf. already G. Garbini and M.A. al-Iryânî in AION, 30 [1970], p. 405 [four times] and pl. I-III) are so well-known that they are maintained here as in the past.

3 - Most of the texts of the present collection are referred to according to the numbers given by al-Iryânî, viz. Ir 41-68. The eleven remaining sigla which are published exclusively by Garbini are also given numbers of his collection, viz. Ga 95-105. It is much easier and much less cumbersome to refer, e.g., to "Ir 42" and "Ga 96" rather than to "Y.85.AQ/16" and "Y.85.AQ/22," respectively.

4 - The author of the color photos now at my disposal always writes "W. al-Aqul," thus, Wâdî al-ʿAqûl.

II - General considerations.

A - From a material viewpoint, there is a disproportion between the il-

lustrations of the two sections and their utilization in the text, viz. the 62 photos + 36 figures pertaining to archaeology are dealt with in only 29 pages, viz.27 pages = 26 Ar pages, + 2 pages (pp. 52-53), but the 34 photos relating to epigraphy are given 60 pages of comment, viz. 20 pages (pp. 21-40) + 40 Ar pages (Ar pp. 41-75).

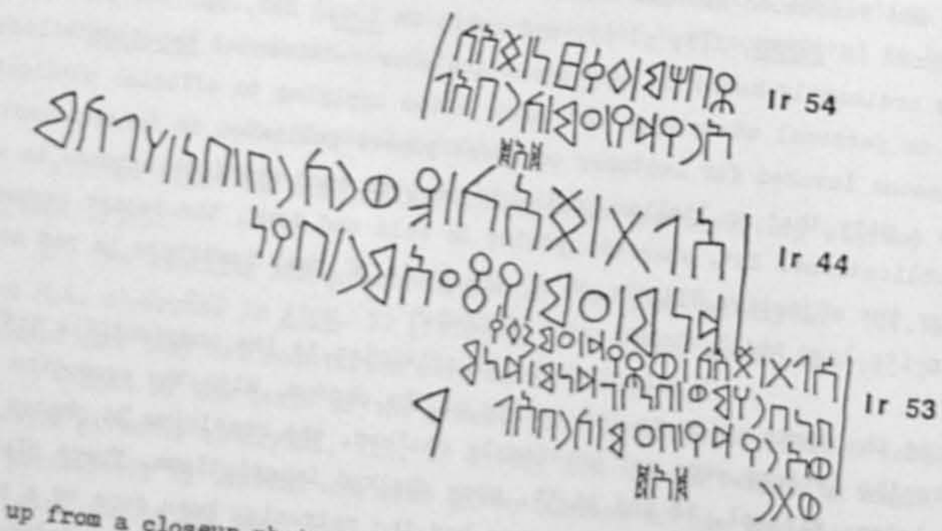
B - A basic criticism of the book is the choice of the language retained by the editors. It is regrettable that the official publication of an Italian institute, whose members are Italians, dealing with the archaeological work conducted by Italians under the auspices of the same Italian institute, and paid for with Italian lire, should be published in English instead of Italian, as if Italian was not an international language duly recognized in the scientific community. The very same policy will also apply to "another volume of this series dedicated to research relative to prehistory and protohistory of the Yemen" and "is planned for the beginning of 1989" (cf. footnote 1 of p. IX). If an Italian institute, of which the members belong to several Italian universities, shows so little respect for its own language and heritage, how can Italy expect its own language and heritage to be honored abroad? The same attitude was also taken by W.W.Müller who renounced his own German heritage in the publication of SDDS; it was denounced in JaMAR, XIV, p. 69, and again in l.c., XVI, pp. 311-312. The lame excuses ordinarily heard in such cases are poor sentimental pro domo's leaning heavily on personal advantages and quite often applying to official publications the reasons invoked for lectures or short papers published in foreign periodicals. It is a pity that an Italian institute adopted such specious excuses in some of its publications. But, when everything is said and done, the reader cannot but wonder why the adjective "Italiano" of the title of that institute is not accounted for in its logo which should read "IsIMEO."

C - Another basic criticism is the unscientific method of chalking the texts prior to the taking of the photos. With the exception of pl. 13 where the letters were not previously chalked, the remaining 24 photos covering 15 plates, viz. pl. 14 and 38-51, show chalked inscriptions. These plates are deprived of any scientific value even had the retracing been done by a professional epigraphist. But, a true epigraphist would never in the least deface a text voluntarily. J. Pirenne acknowledges having also tampered with her photo of PiDPH 23 = Ja 2457 in Raydān, 4 (1982), p. 230: "je donne ici ma photo, où j'ai repassé les lettres visibles," and she, who always wants to convey the impression that she is the palaeographer in SA, is so incompetent that she released her indeniably erroneous

reading of "wʾḪ/ḪḎS/WSRḤŠSWW/wʾMTʿN/ḏ" instead of

wʾḫṭs /wšrḥtsww/wʾmtʿsm (cf. JaMAR, XV, p. 43, where the two following typing errors remained uncorrected in a few copies, viz. "ʾmtʿs/w" to ʾmtʿsm, and "3-4" to 3-5).

The chalking of the texts is unjustifiable for the simple reason that if de Maigret who, as Garbini puts it, "has had no specialized training in epigraphy" (p. 21; cf. below, p. 5) recognized the letter forms and trace them, they could not have been illegible, weathered away or otherwise badly damaged, in which case his chalking would have been mere scribblings. The preceding remark is confirmed by pl. 13 which proves that the texts did not have to be chalked to be readable. It would have sufficed to cut off the 9.5 cm of useless imagery (4.5 from the top and 5 cm from the bottom) and to enlarge each half of the remaining surface (17x8.5 cm) twice its actual size. The same remarks are also valid for pl. 14 which presents 13 cm of useless imagery, viz. 4 cm of the upper segment and 9 of the lower half. I had the inscribed section of pl. 13 photographed in three overlapping sections, and each section was enlarged thrice. On those blown-up prints, most of the letters are legible. Here follows my tracing of pl. 46, viz. Ir 44, 53 and 54, made from the enlargement of pl. 13:



A tracing made up from a closeup photo would be more refined and therefore more accurate, but such a photo does not exist among the 523 (nos.850002-850525) deposited in IsMEO's archives (pp. 57-59).

It is hard to believe that a scientific mission not take close-ups of all those rock inscriptions and have them published without being studied in situ by an epigraphist.

That chalking initiative also leads to an embarassing evidence. The photo of Ir 46 a, b on pl. 40b was taken after a second chalking session: the two extreme letters of b/1, rʿ, are missing from the photo of pl. 41. Why was the photo of pl. 40b published at all if not to fill the lower half of pl. 40 which would otherwise have remained blank? But why was the left side of the photo of pl. 41 not cut off after the decision was taken to print the photo of pl. 40b in order to avoid such an embarassing duplicate? Of course, we take for granted that which is by no means certain, viz. that the difference in contents was noticed.

Garbini's utilitarian pro domo in favor of the chalking process reads as follows (p. 21):

> "The rock inscriptions at al-ʿAql and al-Ǧafnah would have been illegible if Prof. de Maigret had not picked them out with white chalk. In most cases the chalk marks match the engraved characters perfectly, showing the skill of an archaeologist who has had no specialized training in epigraphy. There are, however, a few cases in which the use of several different photographs and the sense of the epigraph itself have facilitated the interpretation of some additional characters, while others have been given different readings. However, I have not noted the few cases of difference between the reading given in the text and that suggested by the chalk tracings."

1 - Garbini's initial statement is contradicted by the simple fact that de Maigret traced them, by pl. 13 (cf. above) and also by his own affirmation of p. 21 (three lines below his above-quoted statement) according to which "not all [texts are] in a satisfactory state of preservation (pl. 38)." This affirmation implies that most of the texts are legible, and the reference to pl. 38 is a window dressing because it shows only chalked inscriptions.

2 - Garbini's phrasaeology contains an obvious contradiction. If the letters in their natural state, viz. before being chalked, actually are "illegible," how can he state that "in most cases the chalk marks match the engraved characters perfectly," and also how could "several different photographs" "have facilitated the interpretation of some additional characters"? If those letters are "illegible," nobody - not even Garbini - can see that the chalking is "perfectly" done and especially since he worked from photos; and no photo taken before the chalking sessions could "have facilitated" their reading. Some expressions may be restored, but not read, on the basis of a well-established pericope. Garbini's understandable wish to compliment de Maigret for his defacement cannot excuse his irrational phrasing which only indicates how blinding his wish to flatter one of his benefactors really is.

3 - Another contradiction which reveals an unscientific attitude lies in the fact

reading of "wʾḪ/BḎS/WSRḤŠSWW/wʾMTʿN/ḏ" instead of wʾḫṭs /wšrḫtsww/wʾmtʿsm (cf. JaMAR, XV, p. 43, where the two following typing errors remained uncorrected in a few copies, viz. "ʾmtʿs/w" to ʾmtʿsm, and "3-4" to 3-5).

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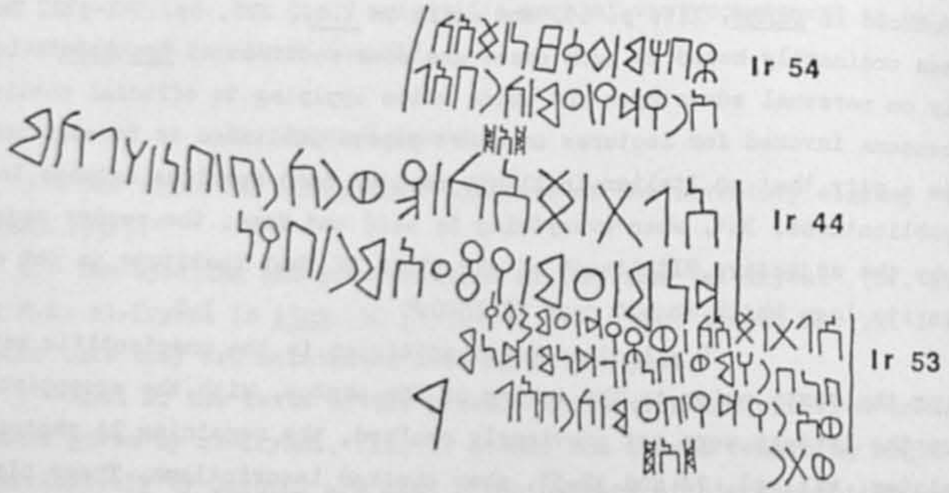

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that, if his initial statement is true, his affirmation on the "several sets of excellent photographs" (p. 21; five lines above the above-quoted prose; the underlining is mine) put at his disposal by de Maigret is a bluff. They may be "excellent" from a strictly photographic viewpoint; but they still are useless if they show "illegible letters." But, if by any chance they are "excellent" because the letters are readable, they should have been published instead of those worthless photos showing chalked texts. Then, the choice of the latter for publication was doubly deliberate and implied the intentional denial of the basic principle of research, viz. the respect of the texts as they are.

4 - The same toadyish attitude led Garbini to yet another distortion of the facts when he speaks of "the few cases of difference between the reading given in the text and that suggested by the chalk tracings" which "I have not noted." That Garbini did not wish to publish the list of those differences is understandable: it prevents the reader from knowing the exact number of those cases. His claim of "the few cases of difference" is nonetheless irresponsible because the reader can easily tally those differences himself. Here follows their list which presents the readings of the plates followed by those of Garbini introduced by "but."

Ir 43 (pl. 45) /3:"ʾmtnw" but (p. 31) "ʾm̊thw;" "ʾryd^{c}" but "ʾrydy;" "^{c}dnm" but "wdnm;" isolated curved lines but "20"

Ir 44 (pl. 46) /1: "gbn" but (p. 31) "/bn;" /2: "yt.ʾmr" but "yt^{c}ʾmr"

Ir 46b (pl. 40b) /1: "w(š)ʾbt" but (p. 25) "ws$_2$ʾbt;" "w..r^{c}" but "wdr^{c}h;" /2: "mh" but "my;" /4: "..dnm" but "dnm"

Ir 49 (pl. 41) /2: "?" but (p. 27) nothing.

Ir 50 (pl. 39b) /1: "/lt" but (p. 24) "ʾlt;" "ś.b" but "s$_3$ʾk;" /3: "hlsl" but "hls$_1$m;" /4: "ʾ.d." but "ʾl/d"

Ir 51 (pl. 39b) /4: "dnl" but (p. 23) "d̊nm;" "ʾry" but "ʾry[d]y"

Ir 52b (pl. 39b) /1: "ʾ/t" but (p. 24) "ʾlt;" /3: "n." but "bn"

Ir 53 (pl. 46) /2: "brn" but (p. 32) "brh"

Ir 55b (pl. 47c) /1: "śṣk" but (p. 33) "s$_3$ʾk;" "^{c}yd" but "ṣyd;" /2: "ʾr^{c}dy" but "ʾrydy;" /3: no word divider after ʾbhw but "/"

Ir 56b (pl.43) /2: "...]nt" but (p. 30) "ʾlt"

Ir 57 (pl. 41) /1: "śṣk" but (p. 29) "s$_3$ʾk;" "g/rs/m" but "br/d;" /2: "wtr" but "/wtr;" "....^{c}m" but "wʾrydy/200"

Ir 58 (pl. 41) /1: "ʾd/i^{c}ly" but (p. 29) "ʾm̊r^{c}ly;" "ylsy" but "ylʾy;" /2: "/w..." but "/wtr"

Ir 59 (pl. 44) /1:"ʾskr/g" but (p. 30) "ʾbkrb;" /2: "yt^{c}.nmr" but "yt^{c}ʾmr;" /3:

"wnlt" but "wʾlt"

Ir 60 (pl. 42) /1: "kšb." but (p. 29) "krb/;" /2: "^{c}t.ʾmrnb^{c}./" but "yt[^{c}]ʾmr/bym;" /3: "wg^{c}m" but "wb^{c}m"

Ir 61 (pl. 39a) /5: "h(?)" but (p. 22) "d"

Ir 62 (pl. 41): "^{c}m/ns" but (p. 28) "w^{c}mʾns"

Ir 63 (pl. 47a) /1: "g/n" but (p. 32) "bn;" /2: "dmr^{c}l." but "dmr^{c}l̊[y]"

Ir 64 (pl. 48b) /2: "ṣt(?)/(?)" but (p. 34) "ṣ..."

Ir 66b (pl. 49a) /1: "ḫlḫm " but (p. 35) "ḫlḫlm"

Ir 67b (pl. 49b) /1: "sbm" but (p. 35) "ʾbm"

Ga 95 (pl. 41):"mr" but (p. 28) "mf"

Ga 97a (pl. 47b): "(ḫ)lrn" but (p. 33) "ʾrn"

Ga 97b (pl. 47b) /1: "tʾ" but (p. 33) "/tʾ;" /2: "lt^{c}..." but "yt^{c}[ʾmr];" /3:"^{c}dy" but "ydy;" /4: "...w..." but "10 (+...)"

Ga 100 (pl. 50a) /1: "yqh" but (p. 36) "yqḫ"

Even gifted with the most vivid imagination nobody can characterize as "the few cases of difference" a long list of more than 80 differences affecting 24 out of the 39 texts from al-^{c}Aqûl and al-Jafnat, thus almost the 2/3 of the texts.

D - The plates.

The survey of the plates reveals more irregularities.

1 - The "List of Plates" (pp. 57-59).

Footnote of pl. 57 states that "the photographs are by" A. de Maigret, V. Francaviglia and A. Solazzi. These persons contributed each with 82, 9 and 4 photos, respectively. However, the footnote omits to mention A. Lentini, author of the last photo of pl. 56, of the last "Appendix" (pp. 52-53) and of the figures 34-36 (p. 56); his name is also missing among the collaborators to the volume (cf., e.g., p. XVII). In the description of pl. 41 (p. 59), add "6" before the six numbers listed; in that of pl. 46 (p. 59), add "(21)" after the three numbers listed.

2 - The plates on epigraphy.

a - Preliminary remarks.

Among the 34 photos dealing with epigraphy, which were all taken by de Maigret with the sole exception of pl. 54b which comes from Solazzi, only pl. 39a and 40a have each the same scale composed of 20 squares of 1x1 cm each.The letters of these two texts, Ir 61 and 65, are about half the size of the others; cf. below, de Maigret's museographic information. The texts are not identified in the captions of pl. 11b, 13, 14 and 38 although it would have been so easy to indicate

the location of each plate involved with a small arrow and a number, and the respective place of each text would be known immediately. It is pitiful to see so many oversized photos (e.g., pl. 39a, 40a, 42, 43, etc) uselessly taking so much space which could have been put to a much better use by the publication of the enlarged close-ups of unchalked inscriptions.

b - Description of the plates.

Pl. 11b: left: general area of Ir 50, 51, 52; cf. also pl. 12; right half = pl. 38.

Pl. 12: close-up of pl. 11b, left fourth.

Pl. 13: left = pl. 45; center = pl. 46; right = pl. 47a.

Pl. 14: left = pl. 38,two left panels; the signs "④," "⑤," "⑥" and "⑧" are traced with chalk above Ir 46, above the end of Ir 41, above "s/f" of Ir 62, and below the beginning of Ga 95, respectively; I do not see "⑦" and did not find a note in the book explaining those numbers; right = pl. 42, left half.

Pl. 38: left = pl. 41; center = pl. 42; right row from left to right = pl. 44, 45, 46 and 47a. Is pl. 43 immediately to the right of pl. 42 and to the left of pl. 44?

Pl. 39a = Ir 61; unknown location in the collection.

Pl. 39b; cf. pl. 11b, left and 12, upper left = Ir 51; upper right = Ir 50; and lower half = Ir 52 a, b.

Pl. 40a = Ir 65; unknown location in the collection.

Pl. 40b = extreme left of pl. 14, 38 and 41; = Ir 46 a, b.

Pl. 41 = pl. 14, left, and 38, left; right section, from top to bottom: Ir 41, 49, Ga 95, Ir 62 and 57; left section: Ir 46 a, b (above) and 58 (below).

Pl. 42, cf. pl. 14, right section; = pl. 38, lower center; = Ir 60.

Pl. 43 = Ir 56 a, b; unknown location in the collection.

Pl. 44 = pl. 38, first left of the right series; = Ir 59.

Pl. 45 = pl. 13, left; = pl. 38, second left of the right series; top = Ir 42; extreme right center = end of Ir 63; bottom = Ir 43.

Pl. 46 = pl. 13, center = pl. 38, second right of the right series; from top to bottom = Ir 54, 44 and 53; extreme right center = end of Ir 63.

Pl. 47a = pl. 13, extreme right = pl. 38, extreme right of the right series; = Ir 63.

Pl. 47b = Ga 97 a, b; unknown location in the collection.

Pl. 47c = Ir 55 a, b; unknown location in the collection.

Pl. 48a = Ga 98; to the right of pl. 47c; unknown location in the collection.

Pl. 48b = Ir 64; unknown location in the collection.

Pl. 48c = Ir 45; unknown location in the collection.

Pl. 49a = Ir 66 a, b; for the location, cf. below.

Pl. 49b: from top to bottom: Ir 67 a, ab, and Ga 99; for the location, cf. below.

Pl. 50a = Ga 100; for the location, cf. below.

Pl. 50b = Ga 101; for the location, cf. below.

Pl. 51a = Ga 102; for the location, cf. below.

Pl. 51b = Ir 68; for the location, cf. below.

Pl. 52 = Ir 47.

Pl. 53a = Ga 103.

Pl. 53b = Ir 48, right.

Pl. 54a = Ir 48, left.

Pl. 54b = Ir 48, complete.

Pl. 55a (photo upside down) = Ga 104.

Pl. 55b = Ga 105.

c - Texts in the plates.

Ir 41, pl. 14, 38, 41	Ir 54, pl. 13, 38, 46	Ir 67a, b, pl. 49b
Ir 42, pl. 13, 38, 45	Ir 55a, b, pl. 47c	Ir 68, pl. 51b
Ir 43, pl. 13, 38, 45	Ir 56a, b, pl. 43	Ga 95, pl. 14, 38, 41
Ir 44, pl. 13,38,(45),46	Ir 57, pl. 14, 38, 41	Ga 96 ——
Ir 45, pl. 48c	Ir 58, pl. 14, 38, 41	Ga 97a, b, pl. 47b
Ir 46a, b, pl.14,38,40b,41	Ir 59, pl. 38, 44	Ga 98, pl. 48a
Ir 47, pl. 52	Ir 60, pl. 14, 38, 42	Ga 99, pl. 49b
Ir 48, pl. 53b, 54a, b	Ir 61, pl. 39a	Ga 100, pl. 50a
Ir 49, pl. 14, 38, 41	Ir 62, pl. 14, 38, 41	Ga 101, pl. 50b
Ir 50, pl. 11b, 12, 39b	Ir 63, pl.13,38,(46),47a	Ga 102, pl. 51a
Ir 51, pl. 11b, 12, 39b	Ir 64, pl. 48b	Ga 103, pl. 53a
Ir 52a, b, pl. 11b,12,39b	Ir 65, pl. 40a	Ga 104, pl. 55a
Ir 53, pl.13, 38, 46	Ir 66a, b, pl. 49a	Ga 105, pl. 55b

The seven sigla Ir 46, 52, 55, 56, 66, 67 and Ga 97 are listed above as composed of two texts each, although Garbini publishes them as containing only one inscription each (pp. 25, 24, 33, 30, 35, 35 and 33, respectively) as al-Iryânî had already done for the first four sigla (Ar pp. 60, 66, 68 and 69, respectively); al-Iryânî does not study the last three sigla, viz. Ir 66, 67 and Ga 97. The preceding division of the texts is demanded in Ir 46, 55, 56 and Ga 97 by the manifest difference in the letter height. Furthermore, the authors of Ir 55b and 56b belong to the krbʾl/wtr's family, viz. as his son and his brother, respectively,

and both participated in krbʾl/wtr's hunts. Therefore, those two persons have nothing to do with ytᶜʾmr/byn whose name is engraved above their text in a.
The division is also justified in Ir 52 and 56 by the senestrograde ductus of a and b/1; were a a part of b, b/1 would normally have been dextrograde.
The division is demanded in Ir 66 and 67 by the difference in palaeography. Besides, btᶜnhn the two Bataᶜites of Ir 66 b/2 calls for two persons, and Garbini's interpretation implies an impossible identity card for ẓwrʾmr would simultaneously belong to two different tribes. He was forced into such an abnormality in order to avoid another abnormality, viz. a personal name of an individual composed of three elements, a feature which existed only during the royal period.
Finally, the space between a and b/1 of Ir 67 demands the separation of each text from the other.

E - The epigraphical material.

1 - The inventory sigla are not mentioned in the Ar section, pp. 7-54. The number "85" is always followed by a dot in the English section, with the exception of al-Iryânî's text which does not use them; it is also omitted in his Ar section with the exception of his nos. 41 and 51-62. al-Iryânî's publication of the texts never refers to the plates.

2 - Dealing with al-ᶜAqûl, de Maigret speaks of "28 Sabaean inscriptions ...(Y.86.AQ/1-28)" (p. 4). But there are only 27 entries from that area published in the book, and they are interpreted by Garbini as "27 ... inscriptions" (p. 21). de Maigret confused the 28 Ir sigla with al-ᶜAqûl although there are only 23 from there since Ir 47, 48 and 66, 67, 68 belong respectively to Yalâ and al-Jafnat; the siglum "Y.85.AQ/28" does not exist in the book. In fact, the editors' 27 sigla of the al-ᶜAqûl material include 32 texts.

3 - de Maigret also writes that "Fig. 5 shows the positions of the inscriptions. The A.85.AQ/1 text [= Ir 61 of pl. 39a] is on the right wall of the gorge about 100m. downstream" (p. 4). In fig. 5, there are 10 small triangles in two rows, 8 and 2 in the upper and lower row, respectively. But where does the counting of his "downstream" start? Does he refer to fig. 5 as it is printed, viz. upside down wih the arrow pointing the north to the bottom? It would have been so simple and so easy to indicate with small arrows the location of each plate on the figures. As it stands, de Maigret's information is practically useless: the identity of the next text, the distance between the two of them and the location of the other texts remain unknown.

Pl. 12 shows the general location of 3 sigla, Ir 50-52 (pl. 39b); on pl. 38, the right section of which is found in unchalked pl. 13, shows the location of

15 more sigla, viz. Ir 41-44, 46, 49, 53, 54, 57-60, 62, 63 and Ga 95. The location of the 16 remaining sigla is still unknown, viz. Ir 45 (pl. 48c), 55 (pl.47c), 56 (pl. 43), 61 (pl. 39a), 64 (pl. 48b), 65 (pl. 40a), 66 (pl. 40a), 67 (pl.49b), 68 (pl. 51b), Ga 96 (no photo), 97 (pl. 47b), 98 (pl. 48a), 99 (pl. 49b), 100 (pl. 50a), 101 (pl. 50b) and 102 (pl. 51a). But Ga 98 is to the right of Ir 55b (cf. below the commentary on Ja 3208). Any attempt at finding the location of these texts is doomed to failure from the start because the Ir sigla do not follow each other logically on the rock. The most complete photo is that of pl. 38 and, e.g., the extreme left complex = pl. 14, left = pl. 41 shows the following disposition of the Ir sigla:

Ir 46 — Ir 41, Ir 49, Ga 95
Ir 58 — Ir 62, Ir 67

Garbini mentions "the order of the epigraphs on the rock, from left to right and from top to bottom" (p. 21). This statement is already contradicted by the preceding schema. Furthermore, on pl. 38 which shows from left to right pl. 41, 42,44, 45, 46 and 47a, I do not find pl. 43 which should normally be on the rectangular-like panel between pl. 42 and 44. As mentioned above, there is no photo illustrating Ga 96 and, since the fact is not pointed out, no reason is given to justify the omission.

The accurate location of the 9 texts from al-Jafnat on pl. 49-51 also remains unknown. The penult paragraph of de Maigret's presentation of the al-Jafnat complex says that "about 300 metres north-west of the basin,... there is a large, fortified rectangular area" (pp. 9-10) of which the plan had not yet been drawn but which is nonetheless described as being a "garrison" (cf. also fig. 1). Incidentally, note that in fig. 1 the coordinates are missing, and "1000 KM" in the lower left corner must be corrected to "1000 MT." Then he states that "7 short inscriptions [= sigla] [are] carved into the rock by tne west gate," viz. Ir 66a, b, 67a, b, 68, Ga 99, 100, 101 and 102. Unfortunately, the only rectangular area of fig. 1 cannot be the "garrison" because the latter is indicated further northwest; and that "garrison" is drawn as a square building. Furthermore, the arrow "inscriptions" points to the southern extremity of the northwestern wall, the southwestern wall being the side of the rocky mountain. Moreover, the plates involved do not suggest that the seven sigla are close to each other. So, the reader still does not know the exact location of those seven sigla. For de Maigret's

statements on the location of the five sigla from Yalâ (pl. 52-55) = 4 texts (Ga 103, Ir 47, 48 and Ja 3207), cf. below, the publication of the inscriptions.

4 - de Maigret's museographic description of the rock inscriptions. The only museographic detail given by de Maigret is that "the characters of the most carefully executed inscriptions measure 12 x 6 cm. on average (pl. 14)" (p. 4). The choice of pl. 14 contradicts the author's palaeographic description because pl. 14 contains one of the most cursive texts of the collection, viz. Ga 95. Furthermore, there is no justification for referring to pl. 14 rather than, e.g., pl. 41. The difference between the two plates is that the scale of the first is much smaller than that of the other and, therefore, the difference of letter height is more difficult to measure. But, the importance of the reference to pl.14 is that the author intended to speak only of the inscriptions relating to the hunting parties (such as Ir 41), not to the other texts, such, e.g., as Ir 61 (pl. 39a) and 65 (pl. 40a), where the letter height and width vary considerably in the same text; e.g., in Ir 61: ʾ: 1.6 x 1.2 cm; s: 1.3 x 1.2 cm; and h: 1.7 x 1.3 cm; and in Ir 65: h: 1.6 x 2 cm; t: 2.1 x 0.7; and t: 1.3 x 1.3 cm. In each group, the first figure is that of the height. In addition, it is rather manifest that the height-width proportion is not 1/2 except in very few cases found in texts engraved by unskillful workers, such, e.g., as the letters thw of Ir 43/3 which the engraver crammed between the crack in the stone and l.2, and also in Ir 44 where the proportion is irregular but mostly vacillates between 1/2 and 1/3. The normal proportion is 1/3 as seen, e.g., in Ir 42, 43, 53 and 54. In some other texts, such as Ir 56a and b, it is closer to 1/3 (cf. the first y of text a: 2.5 x 1.1 cm) than 1/2 (cf. the letter k of text b/1 with the proportion of 1/2: 2.6 x 1.3 cm).

No measurement of isolated stones is available, viz. Ir 47 (pl. 52), Ga 103 (pl. 53a), Ir 48 (pl. 53a, 54a and b), Ga 104 (pl. 55a) and 105 (pl. 55b).

5 - The inscriptions are published without any concordance of the sigla and any word list. The reader is thus forced to do both himself, and, in so doing, he finds out a very unusual (to say the least) situation; viz. al-Iryânî gives his own "Ir" siglum 28 times (23 for al-ʿAqûl, 2 for Yalâ and 3 for al-Jafnat), but publishes 24 of them only. The four sigla excluded are 3 from al-Jafnat (Ir 66, 67, 68) and 1 from al-ʿAqûl (Ir 65). These four and the 11 remaining sigla are published exclusively by Garbini.

al-Iryânî states that there are "about 40 inscriptions" (Ar p. 42) according to his own division of the texts. In fact, there are 34 Ir texts which, added to the 11 remaining Ga texts, to a monogram to which Garbini does not give any siglum, and to an inscription unrecorded by the mission, give the grand total of 47.

al-Iryânî states that there are only 24 important and complete texts, and these are taken up in his study. The four excluded sigla are Ir 65-68 (cf. above, p. 12). Whether complete (e.g., Ir 41) or not (e.g., Ir 46), a text is retained by al-Iryânî if it contains one of the four words ʾrydy, dnm, gyd or sʾk, except the three short complete texts Ir 61, 62 and 63. Such a criterion is unacceptable in a publication of texts because the 17 inscriptions (covered by 15 sigla) and Ja 3208 which are discarded by al-Iryânî belong as much to the three collections of texts as the others. Furthermore, why then did he maintain at all his Ir 65-68 sigla? Since he did, why did he not give his Ir siglum to any of the remaining texts? Summarily, al-Iryânî gave his siglum to 2/3 of the rock inscriptions; did not publish all his selections and left the rejected texts to Garbini for publication. The publication of all those 47 texts follows in the footsteps of H.St.J.B. Philby and A.F.L. Beeston in the al-ʿUqlah texts (cf. A. Jamme, The al-ʿUqlah Texts, Washington, 1963, pp. 5-6), viz. no indication on the location of many inscriptions and selection of the texts picked at random. Garbini respects the numerical order of the inventory sigla, while al-Iryânî follows that of his own Ir sigla; and the order of the plates follows that of Garbini's publication.

The preceding remarks already mentioned another novelty, viz. the presence of two different studies of the same collection of texts published side by side. al-Iryânî attempts to justify the initiative as follows: "the more ideas available on the new facts ... the easier one will come to definitive conclusions" (p. 41). What is to be gained from ideas irrelevant to the texts and from two sets of loose readings based on many unreliable photos except confusion and negative extra burden for research? Here follow a few examples.

Ir 41/2 (Ar p. 55; Garbini, p. 26; pl. 41) and Ir 58/3 (Ar p. 70; Garbini, p. 29; pl. 41): ʾrydy in al-Iryânî, but ʾrdy in Garbini. Although Garbini draws the reader's attention upon the defective writing in the first text, which he also finds in the second, his reading is by no means ascertained. The space between the two letters rd of the first reference is about twice as wide as any other of the inscription; thus, the letter y is possible. In Ir 58/3, the width of ʾ is twice that of each following letter; and this suggests that two letters were telescoped into one, and "r" of the chalking could be a misrepresentation of y, as "r" of Ir 57 (just to the right of Ir 58) is for n (cf. below). Nonetheless, al-Iryânî's reading of ʾrydy in both places could well be a mere repetition of the full form ascertained in 11 other texts.

Ir 42 (Ar p. 57; Garbini, p. 30; pl. 13 and 45). The left edge is traced as a single line in al-Iryânî, and is described as such by Garbini. But, it is composed of two unequal parts on pl. 45, viz. a very short line above a long one. In al-Iryânî's tracing of the outside symbol, the two horizontal lines are missing.

Ir 46b (Ar p. 61; Garbini, p. 25; pl. 40b). The beginning is read "ws$\acute{}$ʾkt" by al-Iryânî, but as "ws$_2$ʾbt" by Garbini as in pl. 40b, which has been chalked. al-Iryânî most probably transferred here the feminine verbal form s$\acute{}$ʾkt of Ir 43/1.

Ir 48 (pl. 53b a,d 54 a and b). al-Iryânî does not use the right fragment = pl.53b and 54b.

	al-Iryânî, Ar p. 62	Garbini, p. 38	Correct reading
1.1	... hqnyy/ʾlmqh ṣlmnh..	ḏnt/mwd hqny/lʾlmqh ʿlm....	ḏnt/mwd hqny/ʾlmqh ʿlmnʿhʾ[n]
1.2	...	hqmnh/lẖlfh...	hqmh/lẖlfṯh][w/...
1.3	...nṯ	...wṯhw/whnbṯ	...mẖthw/whnbṯ
1.4	../ʾrẖhw	.../n̂ʾrẖhw	...]ṯ/bʃʾrẖhw
1.4	/bʿ...	/bʿṯ[t]r/wb	/bʿṯtr/[w]b

For the rather spectacular case of Ir 47, cf. below, the study of the text.

The two publications of the material fail to give the necessary information on each text; they also are saturated with ideas foreign or irrelevant to the SA material. Garbini is loaded with such statements or deductions based on a superficial analysis of the inscriptions as well as on an incompetent knowledge of the material; e.g., his position on "The North-Arabic type" of letters (as, e.g., in Ir 50, 52 and 53), and he does not even care - provided he knows it - to specify which NA lettering he has in mind. Many of those statements will be taken up in the following pages.

The sad reality is that the chalked photographic documentation is the worst material possible; its unscientific value makes it impossible to properly study the inscriptions. Unfortunately, there presently is no choice whatsoever. Who indeed can predict when (if ever) IsMEO will publish a set of useful photos which would at long last do justice to its findings?

III - The stemmas of the two mukarribs and their followers mentioned in the rock inscriptions.

The stemmas of the two mukarribs yṯʿʾmr/byn and krbʾl/wtr and of the persons listed in the collection of rock inscriptions in relation with the two rulers follow a series of introductory notes. The collection of the al-ʿAqûl rock texts appears to represent an historical page revolving around two mukarribs and centered on some of their hunting activities. The lettering of those texts also belongs to

one period in which both the boustrophedon and senestrograde ductus were still used side by side: the choice of one ductus over the other was up to the engraver. The unity of that collection justifies the identification of one ruler mentioned with his identity card with his homonym without it because, as usual, the identity card of a mukarrib is found only in his own inscriptions and nowhere else.

A - The meaning of the word mkrb.

In his book entitled South Arabian Hunt (London, 1976, p. 109, note 358), R. B. Serjeant notes that "I incline also to Miss Pirenne's idea that the mkrb might be rendered the 'unifier' of the country" without any reference although it is the only mention of her name in the book according to p. 134B. Furthermore, his opinion remains unjustified and, therefore, may be attributed to personal reasons which are unacceptable. The same attitude is also displayed by A.F.L. Beeston in 1981(cf. R.C. Stiegner, Al-Hudhud. Festschrift Maria Höfner zum 80. Geburtstag, Vienne,1981, p. 24); cf. already JaMAR, XII (1982), p. 71, note 23. Both Serjeant and Beeston tread in G. Ryckmans' footsteps (cf. Histoire générale des religions [Aristide Quillet, 1960], Paris, II, pp. 209B and 596, note 210). The two aforementioned authors refer to PiPISA, pp. 144-146. Both PiPISA (p. 145, footnote 2) and G. Ryckmans emphasize that she had already mentioned her new translation after a lecture given in Leuven in March 1955, in an answer to R. de Langhe's prearranged question, which had been the main reason for her being invited to that Colloquium Biblicum Lovaniense. Both PiPISA's and G.Ryckmans' obvious purpose was to prove that Pirenne had been the first to suggest that interpretation because both refer to W. Phillips, Qataban and Sheba, New York, 1955, immediately after their statement. G. Ryckmans copied his information from Pirenne. The latter's reference to "p. 108" of Phillips' book is accurate because she utilizes the London edition (p. 317). G. Ryckmans, in an effort to show that he is very complete, transcribes also the subtitle as "Exploring Ancient Kingdoms on the Biblical Spice Routes of Sheba" (p. 604B); but this copy contains two errors, viz. the omission of the article before "Ancient," and the substitution of "Sheba" to "Arabia." G. Ryckmans also manifested his desire of being complete by mentioning the two edition places, "New York et Londres;" after that, he kept on copying Pirenne; thus, his "p. 108" is erroneous for in the first edition he cites, that of New York, the correct reference is "p. 107." More importantly, as already stated thirteen years ago in JaMAR, I (1976), which both Serjeant and Beeston prefer to ignore, Pirenne's lecture in Leuven took place Thursday, "le 31 mars 1955" (cf. Ephemerides Theologicae Lovanienses, 31 [1955], p.697). The publication date of W. Phillips' Qataban and Sheba (New York, 1955) is Friday,

February 25, 1955. My whole argumentation on mkrb is to be found in Cahiers de Byrsa, 5 (1955), pp. 271-272 (forty lines), a publication which Pirenne has steadfastly ignored, including in PiPISA, p. 315.

B - yṯʿʾmr/byn's family.

1 - ʾbkrb (Ir 59), brother of yṯʿʾmr/byn. - Garbini does not identify the father of ʾbkrb whom he declares to be "also a prince of the blood" (p. 30). The adverb "also" refers to the commentary on Ir 56 (p. 30) which immediately precedes Ir 59 in Garbini's paper (cf. below, p. 52).

2 - ḏmrkrb (Ir 60), son of yṯʿʾmr/byn. - Garbini's commentary on this name is the clearest and most important presentation of his views because it involves no less than five texts. It reads as follows (pp. 29-30):

> " *ḏmrkrb* – a name already recorded. He is probably the son of the *mukarrib*. He certainly belonged, as did the authors of AQ/11, 14, 15 and 21, to the group of families from which the Sabaean sovereigns were chosen. The mention of both the *mukarribs* is to be noted. "

The 1st phrase means only that the name is not new in SA.

The 2nd phrase justifies the question mark in my reconstruction of yṯʿʾmr/byn's stemma according to Garbini (cf. below, p. 23).

The 3rd phrase practically eliminates the hypothesis of the 2nd phrase; therefore, the name is put between brackets in my above-mentioned stemma.

But, let us first collate the data of Garbini's commentaries on the four referred-to texts, viz.

in Ir 56b, mʿdkrb is declared to be " prince" because "his father bears a mukarrib' [sic!] name" (p. 30);

in Ir 57, hlkm is said to be "probably a prince of the blood" because ḏmrʿ[ly], which is his father's name, "is a name borne by the mukarribs" (p. 29); thus, the same reason as above, in Ir 56b;

in Ir 59, ʾbkrb is presented as "a prince of the blood" (p. 30; as in Ir 57); but no remark is made on his father's name; and

in Ir 63, lḥyʿṯt: "given the general context, it is probable that "he was the son of one of the nobles from whose ranks the sovereign was chosen" because ḏmrʿly, his father's name, "was typical of the mukarribs" (p. 32).

The following remarks are in order.

(1) It is astonishing that the very simple formula used in all texts could give rise to all those different interpretations.

(2) In Garbini's phrasaeology, "prince" = "prince of the blood" means a member of those "nobles..." = "the group of families..." This remark proves how ambiguous and therefore deceiving Garbini's terminology is because none of the authors of the

five above-mentioned texts actually belongs to the reigning mukarribs' families.

(3) The origin of the mukarribs from those "nobles ..." or "group of families ..." is a completely gratuitous, imaginary scheme.

(4) Garbini does not explain in his commentary on Ir 63 why his idea on the "nobles ..." is not applied to the other authors.

(5) In three out of the five texts listed above, the commentary stresses the importance of what he calls "a dynastic name" (p. 33, on Ir 55b) (cf. below, my study of Garbini's exposé on śʾk, pp. 56-57), a reality which he confuses with "nobles ..."

(6) There is no cross-reference to Ir 60 in any of his commentaries of the four parallel texts chosen by himself.

The 4th phrase points to the most important feature of the text, but his remark remains without any effect on his general understanding of the hunting parties. Had Garbini realized the obvious, it is to be hoped that he might have seen that the first characteristic of his staging of the hunt, viz. "an essential part of their investiture as sovereigns" (cf. below, p. 44) involves an extra improbability, an extra imaginary production, viz. that the two mukarribs would have had to have been elected at about the same time because they would have had their so-called "investiture" at the same time. How thoughtfull it was of the first elected to wait for the second and how kind of him to share his own "investiture" with the other! In addition, the expression "is to be noted" cannot be vaguer, does not say a thing and is used as a real passe-partout in Ir 60 (p. 30): one hunt sponsored by the two mukarribs, and in Ir 59 (p. 30): one individual attending two hunts sponsored each by one mukarrib.

C - krbʾl/wtr's family.

1 - yqhmlk (Ir 55b), son of krbʾl/wtr. Garbini did fortunately not miss the relationship given by yqhmlk twice (ll. 2: bn, and 3: ʾbhw) that krbʾl/wtr was his father. It may be the absence of ʾbhw or of any similar expression in the other texts that prompted Garbini to invent his imaginary theory on the "nobles ..." and "group of families ..." in order to explain the so-called "dynastic name" of several persons. The reason why yqhmlk stated twice his blood relationship with krbʾl/wtr remains unknown. It seems natural, however, to think that his great affection for his father could not be adequately and fully expressed by the cold, formal formula X /bn / X.

2 - ḏrḥn (cf. Ir 68), son of yqhmlk and grandson of krbʾl/wtr. Garbini describes ḏrḥn (p. 36) as "possible ... the son of the author of"

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> " *dmrkrb* – a name already recorded. He is probably the son of the *mukarrib*. He certainly belonged, as did the authors of AQ/11, 14, 15 and 21, to the group of families from which the Sabaean sovereigns were chosen. The mention of both the *mukarribs* is to be noted. "

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2 - drḥn (cf. Ir 68), son of yqhmlk and grandson of krbʾl/wtr. Garbini describes drḥn (p. 36) as "possible ... the son of the author of"

Ga 101, viz. yqhmlk/syḫn. The palaeography of Ir 68 is so close to that of Ir 55b and so remote from that of Ga 101 (with its cursive m) that any relation between Ir 68 and Ga 101 is excluded. Furthermore, no relative of any mukarrib would add the name of a clan or tribe after his name, as yqhmlk of Ga 101 does (for the case of Ir 58/1; cf. below, pp. 71-72).

Ir 68: Garbini, p. 36; pl. 51b; not dealt with by al-Iryânî; for the location of the text, cf. above, p. 11.

smhrym/kn	1	Sumhurîyam has protected
drḫn/bn	2	Darḫân, son of
Wasm yqhmlk	3	Wasm Yaquhmalik.

kn, cf. Ar kanna "to conceal, shelter, protect, preserve" (cf. Wörterbuch der klassischen arabischen Sprache, Wiesbaden, I [1970], p. 370A); cf. also Safaitic, e.g., WH 1631, and Thamudic, e.g., VoEAI-T1, and plural knw in Thamudic JaT 131 a/5. - drḫn, the only person belonging to the second generation of either mukarrib mentioned in the collection, was most probably "protected" or "sheltered" because of his young age. It does not seem probable that he would have been "concealed" from his father during that outing because his father would have been told sooner or later about it by another participant of that excursion (cf. below, commentary on Ir 46b). Garbini takes kn as a personal name. Then, faced with three consecutive personal names, he was forced to declare the text "written by two personages." This suggestion is contradicted by palaeography. - The wasm is understood as "a magical feature"! (cf. below, pp. 32-33).

3 - mᶜdkrb (Ir 56b), brother of krbʾl/wtr. Cf. above, p.16; for the text, cf. below, p. 52.

4 - lḥyᶜtt (Ir 63), brother of krbʾl/wtr. Cf. above, p. 16.

Ir 63: Garbini, p. 32; al-Iryânî, Ar p. 75; to the right of Ir 44, and the end above the beginning of Ir 53; pl. 13, extreme central right; pl. 38, extreme right of the right series; extreme left section in pl. 46, center right; pl. 47a.

lḥyᶜtt/bn	1	Laḥayᶜatat, son of
dmrᶜly	2	Damarᶜalay.

Garbini states that "the last character ... has disappeared and the next to last is only partially intact because of a later piece of graffiti portraying a camel." In fact, the two letters are still clear and given as such by al-Iryânî.

5 - hlkm (Ir 57), brother of krbʾl/wtr.

a - hlkm - Cf. above, p. 16 for Garbini's identification of hlkm.

The first question is the reading of the name of Ir 50/3 (pl. 39b) where al-Iryânî (Ar p. 64) and Garbini (pp. 24 and 25) read, respectively, "hîsl" and "hls,m" = "Hlsm" (p. 24). The name is hlkm in Ir 50/3 as it is in Ir 44/1, 57/1 and 58/1.

The second question is the identification of hlkm of Ir 44 with his homonym of Ir 50, 57 and 58. In his commentary on Ir 44, a text written by ẓwrkrb, Garbini states that the preceding name hlkm "does not appear to be the same person as in AQ/11 and 12" (p. 32) = Ir 57 and 58. The reason of the rejection of that identification is that Ir 44 is not engraved immediately on the left of Ir 58 or immediately on the right of Ir 57, viz., according to Garbini, the texts should queue up in order to prove an identification. However, since his example of qeuing up is a text above the other, Ir 51 and 52a (cf. below, pp.21-22: ʾdbᶜly), the reader still does not know whether Garbini demands a vertical or a horizontal qeuing. In fact, in the case of Ir 44, what is really disturbing Garbini is that Ir 44 with the mention of ytᶜʾmr/byn is splashed in large letters across the whole panel while two dwarf-like texts related to krbʾl/wtr edge the other, above and below respectively. It is, therefore, understandable that Garbini's stemmas (cf. below, p.23) have only the information that cannot be missed. Garbini's position is unacceptable because it would prevent the identification of persons mentioned in texts separated from each other; cf., e.g., ṣbḫm in Ir 54/1 and 61/2.

The third remark to be made here is this: although hlkm himself (Ir 57) "is probably a prince of the blood" (cf. above, p. 16), it is quite surprising that his children are presented by Garbini as ordinary persons, and none of them is suggested to be "probably a prince of the blood" as his father. Garbini remains silent on the question as if he had never noticed it.

b - hlkm and dmrᶜly.

The main problem of the stemma of krbʾl/wtr is the relationship between hlkm and dmrᶜly in Ir 57/1. al-Iryânî reads "bn̂" (Ar p. 69) but he notes that that relationship is very doubtful because of the obliteration of the text. His "n̂" therefore practically amounts to a restoration. Garbini, however, claims that "the reading [of br] appears securely established" (p. 29). The chalked photo of pl. 41 (lower right corner) shows "g/rs/m" (cf. above, p. 6), a chalking which not only confirms that two letters and one word divider are more or less obliterated, as al-Iryânî points out, but also shows that the letter type of "r" is unacceptable in the present collection, viz. the upper part of the letter curving up vertically, and the lower part also containing a small curve. It is not possible, therefore, even to consider Garbini's reading and explanation and all the more so because "br" would then be an hapax legomenon in the whole SA material. Thus, the reading of bn must

be retained, whether the second letter should be put between parentheses or brackets or half-brackets; this depends on the reading of a good photo.

The noun brw = bn is very rare in SA; e.g., in the JaSIMB collection of texts, brw is found about 40 times less often than bn. In most of the cases there does not seem to be any difference between the realities covered by the two words. There are cases, however, where the meaning of "son" is unacceptable.

In 1976, in Carnegie Museum 1974-1975 Yemen Expedition (Pittsburgh, PA), I discussed the question of brw (pp. 97-99)in connection with Ja 2856/4. The information given by Ja 2856/4 and Ir 13/8 and 16/2 indicates that brw in the three texts is a person in charge of somebody or something belonging to somebody else. "The general meaning is, thus, 'intendant, administrator, manager'" (p. 99). Then, one year later, in CIASA, I (1977), p. 216, A.F.L. Beeston refers only to W.W. Müller's theory in Neue Ephemeris für semitische Epigraphik, I (1972), p. 98, which had already been rejected by JaMAR, VII (1974), p. 204, as "uncalled for" because Müller's comments inject into the discussion imaginative ideas, such as foster child, etc., of which there is no epigraphical evidence. The ordinary reader would never realize that Beeston's quotation of Müller's text eliminates a comma and eleven words, viz. "oder sich in bn/brw der Gegensatz 'Kind und Kegel' wiederspiegle," and miscopies "eheliche" instead of "ehelichen." But, speculation being the name of Beeston's game, he theorizes that "bn is a term of social organization applicable both to the actual son and at times to an adoptive or foster-son;" and - of course he could not miss the occasion - he brings up an example "in our own society." The principal feature of his theory, however, is that "brw is a biological term denoting the child actually begotten by someone." In fact, Beeston does nothing but reactivate Müller's idea of "an adoptive or foster-son." It is but a vicious circle: he introduces an imaginary hypothesis which he cannot substantiate with a text as the justification of the use of brw. Furthermore, his presentation of the bn case is precisely the reverse of the reality, viz. bn is a concrete word which means first of all a real son and, because of that natural fact, it also means a descendant more remote than the first generation and finally became a term characterizing the consanguinity of the members of either a family or a larger ethnic unit. Beeston's comment on brhw/rḏwm/bn/rḏwm of 1.3 of YM 545, the text he publishes in CIASA, I, is nothing but the repeat of his thesis brw = bn presented in a different manner. It reads as follows:

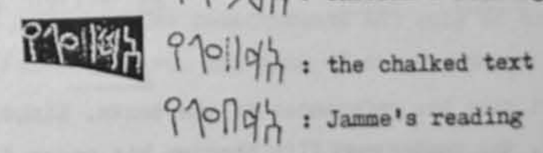
> "When a person's *brw* is mentioned (*brw-hw*), only the child's personal name is usually mentioned, since his patronymic must of necessity be that of the individual to whom the pronoun refers. At first sight, therefore, it is surprising here to find a patronymic BN/RḎWM added to the personal name. One might seek to explain this on the basis that the individual named was the real son (*brw*) of the dedicant, but had adopted the familial name of a (possibly) foster-parent."

It is pathetic to see Beeston struggling to get out of the straitjacket he locked around himself by imagining a fosterling adopting the patronymic of some foster home! Even his first phrase is a personal condemnation: the mention of "usually" implies the existence of exceptions to his own rule. But he fails to inform his reader what they are and what his solution of each case might be. Furthermore, he has no idea of the patronymic of the author of the text since that section is missing. Everything is simple for anyone who refuses to endorse Beeston's straitjacket by giving brw the meaning of "intendant, guardian"
as in Ir 16/2: brw/K.>bnyhw/ʾġwm/bn/d(ḥ)lmlm; compare with
YM 545/3: brw ⁀ hw/rḏwm/bn/rḏwm
Two years later, in Raydān, 2 (1979), p. 98, Beeston's rejoinder to Garbini's objections reiterates his 1977 views but not without drastically altering the whole perspective of his 1977 exposé; and Beeston does not point it out, thus leading his reader to believe that his 1979 argumentation is absolutely identical with that of 1977. The basic difference between the two presentations is that what "at first sight [ ], it is surprising ..." becomes now "in this case, there is nothing unnatural" thanks to another example "in our own society." Therefore, the idea of "an adoptive or foster-son" or of a child who "had adopted the familial name of a (possibly) foster-parent," which was the main thrust of his 1977 reasoning, has vanished in the thin air without even as much as a furtive mention. This is the third time that Beeston is found deliberately claiming to repeat a theory which actually is the opposite of the first; for the first two more dramatic examples,cf. JaMAR, XIV (1985), pp. 80-82: "Beeston's spectacular about-face" and pp. 90-93: "The case of the substitution of a text."

Finally, it is Beeston's 1979 prose that is referred to in SDDS, p. 32, where "p. 99" must be corrected to "p. 98."

c - hlkm's children.

(1) ʾdbʿly (Ir 58). Garbini's only comment on his reading of the name as "ʾmrʿly" is "if the reading is correct" (p. 29):



Garbini should have explained his reading and at least have informed his reader of the differences between his reading and the chalked text (cf. above, pp. 6-7). Although there is no bn in l.1, Garbini says that "it is very probable that" ʾdbᶜly "is the son of Hlkm" of Ir 57 "since the two epigraphs [viz. Ir 57 and 58] almost run together (cf. AQ/2 and 3)" (p. 29). He uses the ambiguous expression "almost run together" instead of "the 2nd text following immediately the 1st at almost the same level." Then, he compares the preceding case with Ir 51 and 52a (cf. above, p. 19) where Ir 51 is above Ir 52a. But this reference to "AQ/2 and 3" proves the intent to deceive because it is Ir 50 and 51 that "almost run together" (cf.pl. 39b) as Ir 57 and 58. Unfortunately, the authors of Ir 50 and 51 are different from each other; thus, Garbini's so-called reference in fact proves the invalidity of his argumentation.

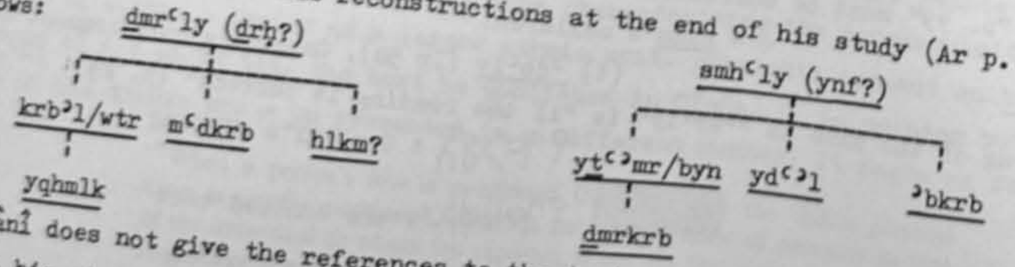
The case of ʾdbᶜly is unique: not only the filiation is not mentioned after the name but also a derivative indicating the place of his origin (not his clanic affiliation; cf. above, p. 18) follows hlkm's name. This name is to be treated as in many proscynemas and rock inscriptions as the father/family name. This double difference between ʾdbᶜly and hlkm's other children seems to suggest that ʾdbᶜly's mother was probably either from Yalâ or living there; thus, ʾdbᶜly would be the half-brother of smhʾmr and ẓwrkrb.

(2) smhʾmr (Ir 50). The author's name is read "s,mhs,mr" or "Smhsmr" by Garbini (p. 24) and smhsmr by al-Iryânî (Ar p. 64). But, the element "smr" remains unknown as a nominal component; on the contrary, ʾmr is frequently used in such nominal derivations. I therefore read the name as smhʾmr. For the correction of the name of the author's father, cf. above, p. 18. Any possibility of affiliation with a mukarrib's family is obviously excluded in Garbini's thesis by his misreading of the name of the author's father.

(3) ẓwrkrb (Ir 44) is the only member of a mukarrib's family who is reported to have beaten up exclusively for the other mukarrib.

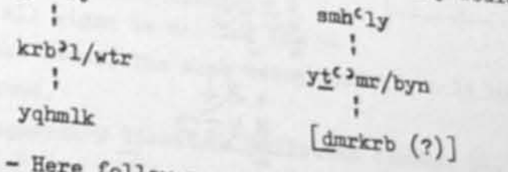
D - al-Iryânî and Garbini's stemmas.

al-Iryânî presents his reconstructions at the end of his study (Ar p. 54) as follows:



al-Iryânî does not give the references to the texts. Since he does not add a word list to his study, the reader must flip through his paper to find the references him-

self. The identifications of the two heads of family and the insertion of ydᶜʾl of Ir 48/1, which does not belong to the group of the rock inscriptions, are the consequences of his theory on the early mukarribs. It is difficult to understand, e.g., why he does not make ytᶜʾmr/byn the nephew of krbʾl/wtr since smhᶜly/ynf is the son of dmrᶜly in RÉS 2651/1 and, therefore, to give six sons to his dmrᶜly. Garbini does not build up the stemmas of the mukarribs. As a matter of fact they would have been almost useless since they would have been reduced to these:



E - Here follow my stemmas of the two mukarribs in the rock inscriptions of the al-ᶜAqûl area and of the participants in their hunting parties; see the following page.

IV - The inscriptions.

A - Ja 2971 F = Ga 103.

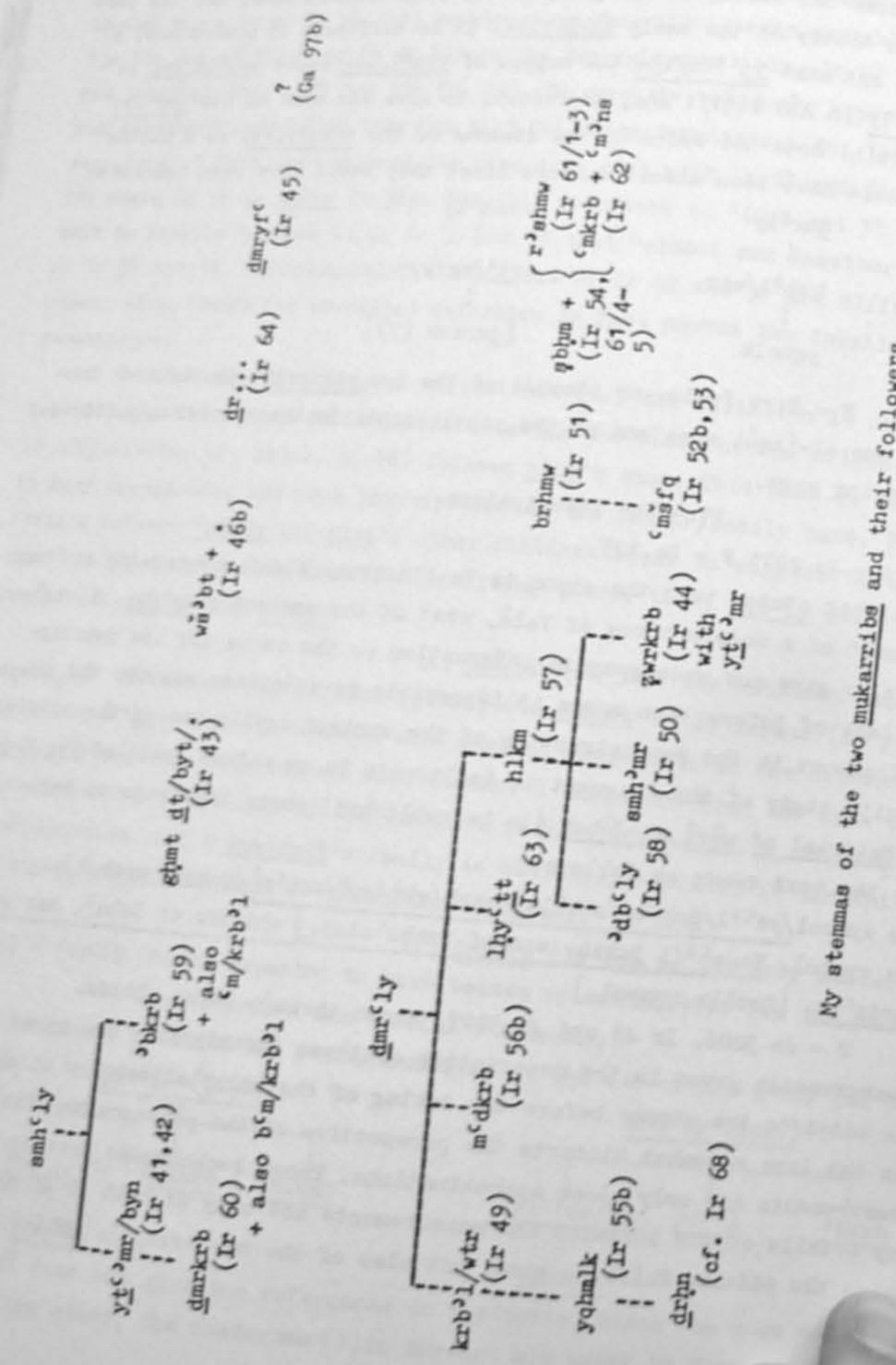
de Maigret states that the stone is "a limestone slab" now serving as "the right doorjamb" of a modern house of Yalâ, west of the ancient city (pp. 13-14), but he fails to give any museographic information on the stone and its inscription. This lack of information makes it impossible to determine exactly the place of the fragment in the reconstruction of the various duplicates of the original. Garbini's study of the fragment is dealt with in my volume entitled The Epigraphical Material of Wâdî al-Jûbah (to be published) where the fragment became Ja 2971 F; the text reads as follows:

[Double symbol/ydᶜʾl/drḥ/bn/smhᶜly]/mkrb/sbʾ/gnʾ/mrdᶜm[/Double symbol]

[Double symbol. Yadaᶜʾil Dariḥ, son of Sumhuᶜalay,] unifier of Sabaʾ, has walled Mardaᶜum. [Double symbol.]

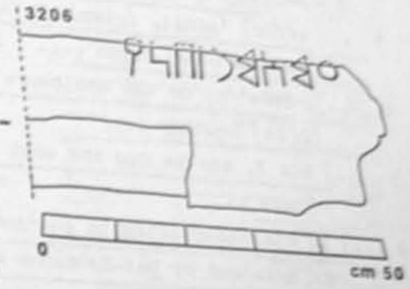
B - Ja 3206, Ir 48 and Ja 3207, known through color slides.

The measurements given in the description of these inscriptions are based on the scale added to the stones before the taking of the color slides now at my disposal. As the lens somewhat distorts the perspective of the photographed field, those measurements are only close approximations. Those kodachromes provide the necessary details on the location and measurements not only of both Ir 48 and Ja 3207, which the edition fails to give, but also of the new text, Ja 3206.



My stemmas of the two mukarribs and their followers.

1 - Ja 3206: a large sandstone block serving as lintel in the door of the ḥiṣn above Hajar Yalâ, the lower half of its left section was hollowed before being reused. Approximate measurements of the section visible on the photo: stone: 55 x (left)12 and 21,(right) 22.5 cm; text: 33.5 x 4.5 cm.

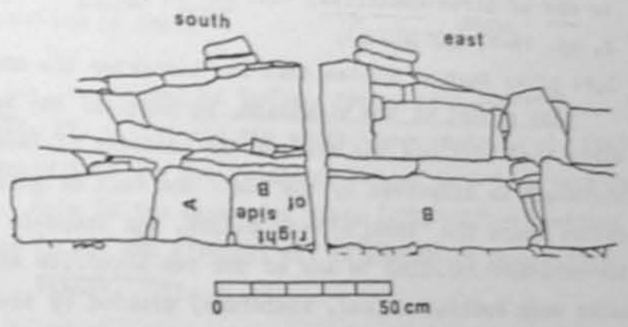


ʿm'mr/bny ʿAmm'amar has built.

The upper part of all signs is missing due to the recutting of the stone. The slab being reused, it is not possible to know which building was involved.

2 - Ir 48: two fragmentary limestone yellowish blocks: pl. 54b; Garbini, pp. 38-40; al-Iryânî, Ar p. 62, with commented translation.

A: the right section (pl. 53b): reused on its left side in the 3rd stone row (from the top) in the southern wall of the ḥiṣn at Hajar Yalâ, about 24 cm from the eastern edge of the wall; these 24 cm are filled by the right lateral side of the left section of the same text; 16 x 19 cm.



B: the left section (pl. 54a): reused upside down in the eastern wall contiguous to the above-mentioned one and immediately against the bottom side of A; 65 x 19 cm. Above is the sketch showing the position of the two fragments. de Maigret gives the following information on the two sections: they are "in [the] south-east corner" of "a modern square house (C), ... about four metres above the ground. The longer is upside-down on the east side of the house. The other is at an angle of 90 degrees on the south wall ... of shelly limestone" (p. 11); for the location of that "house (C)," cf. fig. 15, upper left. The preceding description is both imprecise and incomplete.

1 ← ...]'dnt/mwd/yd'l/wyt'mr/ḥqny/'lmqh/'lmn⌜h⌝[n/...
2 → ...]./yd'l/b'ly/gn'/ḫfry/wygn'nh/whqmh/lḥlf⌜ḫ⌝[w/...
3 ← ...]mḫthw/whnbṭ/b'rn/rwym/b'rydy/wryd/mnq[l...

4 →...]ₗ/bⱼ ʾrḫhw/ʿd/dʾnšmm/bʿttr/[w]b/[ʾ]lmqh/wb/dtₗḥⱼ[mym/wb/dtnʿdnm/wb/...

1 ... of/in] ʾAdnat, friend of Yadaʿʾil and of Yataʿʾamar, has dedicated to ʾIlumquh the (two) signs [...

2 ...]. Yadaʿʾil on the enclosure wall of Ḥafray: he will wall it and erect it for h[is] country ...

3 ...] his ?, and he dug the well Rawyum at ʾArayday; and he desired [a] mountain [pass ...

4 ...] in his undertaking in Dû-ʾAnšamum. By ʿAttar [and] by [ʾI]lumquh and by Dât-Ḥi[myâm and by Dât-Baʿdânum and by ...

For some remarks, cf. above, p. 14.

L.1: dnt: the name of the great Wâdî ʾAdanat at this place of the inscription indicates that the missing beginning was rather long. – mwd/ydʿʾl/wytʿʾmr: this title was borne by each of two Saqrânites of the famous tribe Ḫalil, viz. the two brothers mʿdkrb and ʾbkrb in Ja 2839/12-15; and the son of the first, wddʾl, even became mwd of three mukarribs, viz. ydʿʾl, ytʿʾmr and krbʾl in Ja 2839/16-17 (cf. JaMAR, X, pp. 16-17 and pl. 2).

L.2: ḥfry: Garbini states that it "indicates the name of the city whose walls were the object of the attention, perhaps, of the mukarrib Yadaʿʾil. It is very probable that this was the name of the locality of Yalā itself" (p. 39). The initial statement is disproved by the text: the wall of Ḥafray is mentioned only as the location where the ʿlmnh[n] were placed, and there is no justification in ascribing the intended building to any of the two mukarribs since the text says that the two marks were dedicated (and, therefore) erected by the author of the text. The personal pronoun -h appended to both verbal forms ygnʾn and ḥqm refers to that intended building, not to gnʾ/ḥfry. The probability of Garbini's second statement is gratuitous since the two fragments are not in situ and, therefore, could have been brought there from any other site, as Ja 2971 F = Ga 103 was.

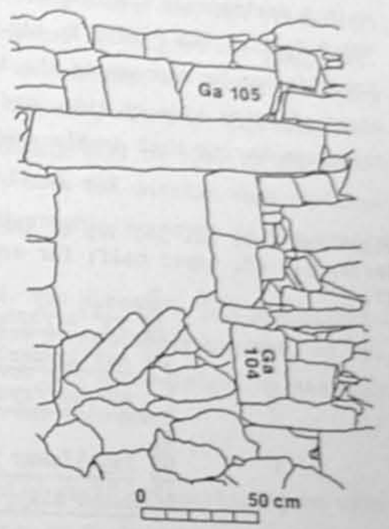
L.3: for ʾrydy, cf. below. – ryd, cf. Ar râda (o) "to desire" (cf. E.W. Lane, An English Lexicon, London, 1867, pp. 1183C-1184A).

L.4: ʾrḫ, cf. JaSIMB, p. 427B. – dʾnšmm, a dʾqtlm nominal derivation; cf. Ar našima "incepit rem et agressus fuit" (cf. FrLAL, III, p. 283B).

3 – Ja 3207= ...+Ga 104+105+...: two fragments of a yellowish limestone broken on their lateral sides, reused in the front wall of Hajar Yalâ; height of each fragment: 22 cm; letter height: 14 cm.

Ga 104: cf. Garbini, p. 40; pl. 55a; as already noted above (p. 9), this photo is upside down; on its right side in the lower half of the right doorjamb; length of the stone: 28 (top) and 33 (bottom) cm.

Ga 105: cf. Garbini, p. 40; pl. 55b; immediately above the lintel of the same door; in the axis of the right doorjamb and in the fifth stone row above Ga 104; length of the stone: 34 (top), 40 (center) and 24 (bottom) cm. Here is the sketch of the door, the entrance of which is partly blocked by a cluster of lose stones. de Maigret's information on the two inscribed stones is that the "Islamic tower" (pl.33a) "door faces west. Its right jambstone incorporates two blocks of limestone"(p. 11). This description must be corrected on the basis of the information given above which describes the situation of the door. The tower is the square building A whose location is shown in fig. 15, upper center.



.../k]br/ḏrʿm/[mⱼwd/ydʿʾl/[... ...lea]der of Ḏarʿum, friend of Yadaʿʾil [...

Cf. Schm/Marib 27/1 (cf. ABADY, IV, p. 191 and the upside-down photo on pl. 36c). The size of the letters suggests that the two fragments belong to a long text inserted in an enclosure wall. ḏrʿm is the name of a place (CIH 342/7-8) settled by the tribe of the same name (e.g., RÉS 4176/4); cf. M. Höfner in Inschriften aus Ṣirwāḥ, Haulān (I. Teil), Vienna, 1973, p. 19.

C – Ir 41, 42, 47 and 49.

These four inscriptions having the two mukarribs as authors are best treated together to avoid distributing their commentary in different places as it is the case in Garbini's paper.

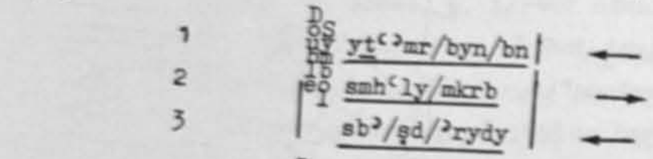
1 – Ir 41, cf. Garbini, pp. 26-27; al-Iryânî, Ar p. 55; pl. 14, upper center; pl. 38, upper section of the second group from the left; pl. 41, upper right; for some remarks, cf. above, p. 13; cf. summary below, p. 41.

1 ytʿʾmr/byn/bn/smhʿly/
2 mkrb/sbʾ/ṣd/dnm/wʾr(y)dy/y
3 wm/ṣd/ṣd/ʿttr/wkrwm/

1 Yataʿʾamar Bayyin, son of Sumhuʿalay,
2 unifier of Sabaʾ, has hunted at Danam and ʾAra(y)day
3 when he hunted on ʿAttar hunting ground and with pits.

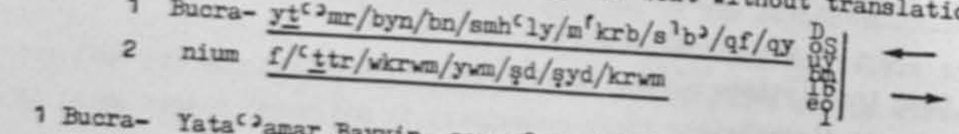
al-Iryânî erroneously indicates with three arrows that the text is boustrophedon with a dextrograde 1.2. In the al-ʿAqûl siglum given by Garbini, correct "95" to "85." Garbini has "ʾrdy" in his transliteration and commentary, but "ʾryd/" in his translation; he represents the two symbols as if they were one above the other instead of being side by side, and he repeats his interpretation in the two other inscriptions having that double symbol, Ir 42 and 49.

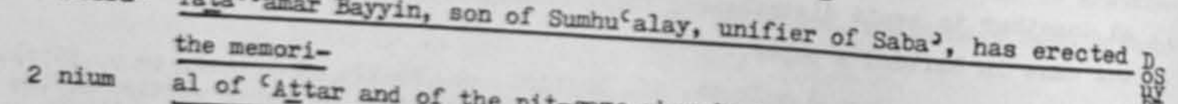
2 - Ir 42: cf. Garbini, p. 30; al-Iryânî, Ar p. 57; pl. 13, upper half of the extreme left: about 2/3 of the right section; pl. 38, top of the second panel from the left in the right series of texts; pl. 45, upper half; for some remarks, cf. above, p. 14; cf. summary below, p. 41.



The upper extremities of the upper symbol are too high on pl. 45; and the lower half of ṣ is clear in pl. 13. For Garbini's representation of the double symbol, cf. above, same page.

3 - Ir 47: cf. Garbini, pp. 37-38; al-Iryânî, Ar p. 61; pl. 52; ABADY, I, p. 79, note 3, pl. 33c; W.W. Müller in l.c., III, pp. 105-107, and in H. von Wissmann, Die Geschichte von Saba ʾ II, Vienne, 1982, p. 103, note 79 (transliteration of the text without translation).





de Maigret states that the stone is "a limestone slab" reused as "the architrave" (p. 13; cf. also Garbini, p. 37; Italian "architrave" means lintel) of a door in a modern house west of Yalâ, as in the case of Ja 2971 P (cf. above, p. 25), where the remark on the location also applies here. - ABADY's photo is of much better quality than that of pl. 52. al-Iryânî describes the bucranium on the right and the double symbol on the left as being each a tuġrâ ʾ, viz. a sultanic intricate signature! Both editors ignore the vertical line to the left of the double symbol; and Müller does not mention anything: bucranium, double symbol and vertical line. Garbini duly notes that the text is "boustrophedon," but Müller does not. Garbini, however, does not even add a single arrow to his transliteration; therefore, the reader still does not know the actual direction of the lines. Neither de Maigret nor ABADY (I, p. 79, footnote 3, and III, pp. 105-107) gives any measurement of either the stone or the letter height. It is incomprehensible that Garbini, although expressly referring to Müller's opinion on krwm published in ABADY, III, p. 105, in his sommentary on Ir 41 (p. 26, ll. 6-7 from the bottom), does not mention that publication in his study of Ir 47 (p. 37). Garbini's most surprising error is his reading of -/q(2)f/qyf/- instead of

-/qf/qy(2)f/-, which is clearly indicated in H. von Wissmann, Die Geschichte..., l.c., p. 103, footnote 79 to which he himself refers incompletely, however, as "p. 103." Since he read the note, which he does however not acknowledge, he had only to copy the reading. But, it is not proven that he read the note!

4 - krwm.

It has become Müller's trademark to literally stuff his publications with all kinds of information, either useless or well-known or gratuitous, relating to the question at hand. Here follow two examples.

(a) He suggests that krwm might be referred to Ar karîm; then krwm could be understood as a qtwl plural form which is well attested in modern Yemeni. But, no translation of krwm is given on the basis of that hypothesis which is simply dropped. Therefore, what is the usefulness of Müller's six lines wasted on his fancy? Furthermore, why could krwm not equal krym, whose plural is kirâm (cf. J. Penrice, A Dictionary and Glossary of the Kor-ân, New York, 1969, p. 125A)? Finally, what does his referred-to Qorân 27/39 (not "40" as given by Müller), fa-ʾinna rabbî ġanîy karîm with the two adjectives "self-sufficient" and "beneficent," have in common with the SA text?

(b) Because of the expression bny/qyf/ʾlmhw of Lu 16+CIH 367/1-2, he has built the stela of his sorrow (cf. my Carnegie..., l.c., p. 153), for which he still maintains the meaning of ritual meal or of cultic gathering, Müller claims that the translation of krwm as "Grubenstele" can hardly be taken into consideration. But, the preceding pericope and qf/qyf/ʿttr/wkrwm of Ir 47/1-2 have only one word in common!

Müller's commentary of more than 100 lines deals exclusively with krwm. It starts (p. 105) with the pericope qf/qyf/ʿttr/wkrwm of Ir 47/1-2 understood as indicating "mit grosser Wahrscheinlichkeit" that krwm also is a divine name. But, he does not justify his verdict. His reasoning looks and must be something like this: both ʿttr

and krwm qualify qyf, and ʿttr is a divine name; thus, krwm must also be a divine name!!! Garbini mentions Müller's opinion on krwm (p. 26) without any reference, and retains it as equally possible as the ordinary interpretation of krw as "traps," which he imaginarily deifies, viz. it becomes "a divine figure personifying the 'trap' or procteting [sic] the hunter in this type of hunt" (p. 26). Müller's argumentation is unrealistic: not only qyf is not synonymous of hqnyt, but it also may be connected with realities which have nothing to do with the divine world. J. Pirenne's study in Semitica, 30 (1980), pp. 93-124 and pl. 4, is grossly steeped in religious obsessions leading her to such statement as qyf "est le signe qui fait deviner le divin, auquel il sert de trace" (p. 122)!!! Regardless of the context in which it is framed, qyf is fundamentally a memorial or a testimony of the action described in the text, whether secular or religious, as the case may be. Müller's commentary where he attempts to explain his thesis, ends up with the translation of Ir 47/1-2 as "... errichtete die Stele des ʿAttar und Karwam, als er die Jagd des Karwam ausübte" (p. 106). Immediately after this translation, however, Müller complements it by saying that the second period may also be translated as follows: "als er das Wild(bret) der Karwam jagdte" (p. 106). Then, the two translations are given as synonymous in RÉS 4177/3-4: -/ywm/ṣd/(4) ṣyd/ʿttr/wkrwm as "als er die Jagd des ʿAttar und Karwam ausübte (bzw. als er das Wildbret des ʿAttar und Karwam jagdte)" (p. 107). Any translation involving divine name(s) and/or animals is contradicted by epigraphy: the verb ṣ(y)d is followed by a place name; cf., e.g., ʿAqabat Kuḥl/2: ṣd/ʾʿrr/ṣwln has hunted in the mountains of Ṣawlân in JaCIASA, p. 268, footnote 21, where it is explicitely stated that "ṣyd/ʿttr et autres locutions suivant immédiatement le verbe ṣ(y)d désignent également des endroits; donc, ṣyd/ʿttr signifie 'le terrain de chasse de ʿAttar' et non 'le gibier de ʿAttar'." Of course, Müller could not care less, cocooned, as he is, in his deep conviction of "being the long-awaited messiah" (cf. JaMAR, XIV [1985], p. 338). Unfortunately, to make things worse, Müller knew that text very well since he himself copied it in 1974, as reported by RycCRA, p. 276. J. Ryckmans could not care less either because all that he is concerned about is that the text, which he himself utilizes, does not specify the kind of animals that were hunted. Therefore, ṣyd/ʿttr means "the hunt of, or owned by, ʿAttar (or His temple)"; and krwm means either the place where the pits to trap the animals were built or the symbol of that particular game. Furthermore, as I have always done in the past, I simply reject the idea of any sacred/ritual hunt in similar texts; cf. recently, JaCIASA, pp. 267-269. It is also the right place to repeat JaCIASA's appropriate remark which reads as follows:

"Ajoutons, pour terminer, que les tenants de la soi-disant chasse rituelle commettent un quatrième « latius hos », celui de croire que les animaux domestiques ou sauvages trouvés sur une propriété appartenant à la divinité lui étaient de ce fait consacrés au point que toute activité les concernant ne pouvait être que rituelle et, s'ils ne retiennent qu'une seule de ces activités comme étant rituelle, savoir la chasse, c'est par pur illogisme."

In addition, the last section of Müller's commentary contains a rather lame explanation of his contribution to SDDS (p. 79, under krw), viz. "n. pr. (Mül)." Moreover, Müller translates hlfy/nwmm of RÉS 4177/3 as "beiden Bergpasse von Nawāmum" (p. 107) without referring to Pirenne's analysis (p. 101) where he picked up the interpretation. Incidentally, this rendering is declared "improbable" by Garbini (p. 37), who suggests the phantastic translation of "the gates of Rest"!!! Finally, in his presentation of the SA religion (cf. W. Daum ed., Yemen [English version], Innsbruck, 1987), which is a summary of the concoctions of the laudemus invicem club members, J. Ryckmans pays his dues to Müller by writing that "another divinity, Kurūm, was the subject of a ritual performed by the ancient mukarribs and kings of Saba" (p. 107B).

5 - Ir 49: cf. Garbini, p. 27; al-Iryânî, Ar p. 63; immediately below and slightly to the right of Ir 41; pl. 14, second from the top of central section; pl. 38, second from the top of the second panel from the left; pl. 41, central right; cf. summary below, p. 41.

1	←	Double Symbol	krbʾl/wtr/bn/dmrʿly/mk
2	→		rb/sbʾ/ṣd/ʾrydy/wdnm/ṯ

1 Karibʾil Watar, son of Damarʿaly, uni-
2 Double Symbol fier of Sabaʾ, has hunted at ʾArayday and Danam: 100.

Garbini's statement on the "very regular script" is another distortion of the epigraphical facts: each line of letters forms a concave line; the letter height varies from 7 to 12 mm (on the plate) so that there are hardly two contiguous letters having the same height; and the top of ṯ is at the horizontal axis of the preceding ʾ. He does not register the number of the tally at the end of 1.2 (cf. already above, p. 6). For his representation of the double symbol, cf. above, commentary on Ir 41 (p. 28). al-Iryânî does not register either the double symbol or the vertical lines on both sides of the inscription, but he duly registers the number of the tally.

6 - The four preceding texts already contain two words well attested in the collection, viz. dnm and ʾrydy.

a - To Garbini's presentation of the two words (p. 23), add Ir 43 to his Ir 49 as mentioning ʾrydy/wdnm. Furthermore, the siglum Ga 97 is not listed under his ʾrydy.

b - Garbini states that the two words "have proved a great obstacle to the understanding of the texts" (p. 23). This is difficult to understand because of the epigraphical fact mentioned above (p. 30) on the true meaning of the word(s) following the verb ṣ(y)d. But, since Garbini, as Müller and Ryckmans, likes to give the impression that he knows everything, it is not after all surprising to see the three of them unaware of that fact. Besides, none of the roots involved, dnm, dw/yn and ryd, yields the name of either a wild animal or a hunting gear unless through some semantic acrobatics. Both words could not be anything but place names, and the proposition b introducing ʾrydy in Ir 48/3 (cf. above, pp. 25-26) cannot but indicate the place where the well rwym was located; for dnm as a place name, cf. already RÉS 3946/3. For ʾrydy in Ir 41/2 and 58/3, cf. above, p. 13.

D - Garbini's commentaries.

Garbini's commentaries on the four above-listed inscriptions contain an impressive number of examples of abuse and distortion of the epigraphical material which enable him to fill up his lines not only with concoctions based only on both his wild imagination and his embryonic knowledge of the problems at hand but also with references to publications of laudemus invicem club members in an effort to secure the continuation of their complimentary silence and approval.

1 - The North-Arabic.

One of the aberrations of Garbini's study is the attribution to the NA lettering (1) of one name of Ir 67 b/1 ("probably");

(2) of a whole inscription, Ga 95 which is called "Teymanite;" and

(3) of 13 letters found in 8 texts, viz. h of Ir 50 (p. 24) and Ir 59 (p. 30); d and h of Ir 51 (p. 23); d and ṯ of Ir 55b (p.33); h and ḫ of Ir 65 (p. 25); d of Ir 68 (p. 36); m, l and n of Ga 101 (p. 36); and m of Ga 102 (p. 36).

a - With the exception of Ga 95 which he ascribes to Thamudic, Garbini never specifies which NA language or alphabet he refers to; neither does he ever list the languages or alphabets involved in his NA.

b - Garbini states that the sign introducing Ir 68/3 "is a magical figure" which "has the form of the character d in North-Arabic" (p. 36). Such form of d is well-known in SA; cf., e.g., Ja 408 a/1 and 2, b, c, d, etc. The very same feature is found below the first two letters of Ga 101, but the sibylline Garbini remains silent: the presence of that sign is not even acknowledged in his commentary.

c - Garbini's two basic methodological errors are not only to base his definite palaeographical study on chalked texts, but also to ignore the hundreds of cursive texts which attest those letter types. Of course, he is in good company with Pirenne, J. Ryckmans, etc, when excluding from SA all texts not engraved in the monumental lettering. Garbini's crass ignorance of the published material is equalled only by his arrogant defiance in publishing his inepties. A single example suffices here. Had he come down from his egoistic pedestal he might have found out that the collection of the rock inscriptions of J. ʿAmûd al-ʿAyraf just a few miles from the al-Jafnat collection already exhibits the letters he singles out; e.g., the name of Ja 2907 d/1 contains both h and ṯ; for the palaeographical presentation of those letters, cf. JaMAR, XI (1980), pp. 16-22.

d - As usual, Garbini does not even utilize the whole material he publishes, viz. he makes no comment on the following letters:

d of Ir 43/3, 52a and 57/2;
h of Ir 52 b/2, 55 b/3, 57/1 and 63/1;
ḫ of Ir 63/1, 67 b/2 and Ga 98; and
n of Ir 53/2.

e - It is not surprising that the form of the 2nd d of Ir 52 b/3, which, according to Garbini (p. 21), "has a peculiar form," did not open his eyes on the value of the chalking because he could not have written his complimentary appraisal of the chalking vandalism (cf. above, p. 5). The reader also wonders why Garbini did not ascribe to his "North-Arabic" not only the above-mentioned letter d but also ś of Ir 59/2 (cf. below, pp. 52-53), for which he uses the equivalent expression, "has a particular form" (p. 30).

f - Ir 67b and Ga 95 are studied below, pp. 86-89.

2 - The dating of the mukarribs.

Garbini places ytʿʾmr/byn "about halfway through the 5th century B.C." on the basis of "the 'short' chronology, which I consider the only valid one" (p. 26) or, as G. Gnoli puts it, "according to a thesis that this authoritative scholar has sustained again and again throughout his studies" (p. XII).

Garbini's profession of faith in the mini-chronology manifests his stubborn rejection not only of all carbon-14 tests and archaeological research made during the recent years favoring the long chronology but also of the information of the present collection. It is a well-known fact that the most fundamental basis of J. Pirenne's palaeographical reconstruction in PiPISA is the letter m either closed in her periods A through C2 or open as of her period C3, as illustrated in her "Tableau 1. Orientation pour l'identification d'une graphie." Since she puts the "fin de la période B, vers 350 avant J.-C." (pp. 137 and 172) and "la fin de la période D ... aux environs de 250" (p. 172), her C3 would probably be around 290 B.C. The information of the present collection confirms, if it was at

all necessary, "la contemporanéité de types dissemblables" already asserted in 1957 (cf. my publication entitled La paléographie sud-arabe de J. Pirenne, Washington, p. 35). The two types of m, closed and open, are attested together, e.g., in Ir 53, and the open m is the only type found, e.g., in Ir 44 and 54; for my tracing of those three texts, cf.above, p.4. Furthermore, the types of Y and Ψ, which Pirenne buries in her "lettres aberrantes" = "signes thamoudéens ou locaux" (pp. 99-100) would exist only in her A3, "A non classés" and B2 (p. 100); they are also well attested in the rock inscriptions from J. ʿAmûd al-ʾAyraf (cf. above, p. 33). Pirenne attempted lately (cf. W. Daum ed., Yemen [English version], Innsbruck, 1987, pp.116-122) to revitalize her mini-chronology. As usual, she takes or leaves whatever pleases or displeases her, and she does not shrink from distorting the facts; e.g., she claims that I did not take the photos of the second panel of the onomastic lists of J. Balaq Janûb. She writes that "as the one on the back stood in the shadow Jamme was unable to make photographs" (p. 120A). Yet, JaMAR, X (November 7, 1979), devoted exclusively to that panel, Ja 2849 c-f, states (p. 2) that "On Thursday January 8, 1976, I devoted the balance of the day, from 10:00 a.m. till 3:58 p.m. [ ] to check and complete my previous reading of the second panel as well as to take more photographs of the inscriptions." Miss Jacqueline Pirenne is so dishonest that the previous information does not exist for her. Or is it dishonesty? I have come to think several times in the past that she was, not dishonest, but irresponsible: her imagination is so big that her reason can do nothing but to follow suit. The preceding interpretation is confirmed by the last statement of her paper which shows not only that she contradicts herself but also that her understanding of the problem is all distorted. She writes: "Might one surmise" that all chronological research on the beginning of Saba by those who do not accept her straitjacket-like system "stems, in the final analysis, from the noble desire to give the Queen of Sheba ... her rightful place in the chronology of world history?" (p. 120B). She does not even see that by the same token she affirms to refuse that queen what she herself declares to be "her rightful place" ... in favor of her dreamy thesis. Yet, in almost every page of her papers she splashes in every possible way, and brags about, her meticulously trustworthy historical method! In addition, the tenants of what she calls "the United States[' .] mixed chronology" (mixed with what?) have never been driven by the lust of sensationalism; and the Queen of Sheba never played any role, real or imaginary, in their study or life, as it seems to sometimes happen ... according to W.W. Müller's stunning testimony in Jemen-Report, 20, Heft 1, 1989, p. 22B. Speaking of the Queen of Sheba, he writes that

" Bei einer solchen Fokussierung der alten Geschichte Südarabiens auf eine einzige Person ist es nicht weiter verwunderlich, daß jemand wie der Verfasser dieser Zeilen, der sich mit dem antiken Jemen beschäftigt, in den *Verdacht* kommt, ein »Liebhaber« der Königin von Saba zu sein. Meine Frau betrachtet sie daher als ihre ärgste Rivalin: Als sie mich bei unserem ersten gemeinsamen Besuch in Marib bat, sie an den Säulen des antiken Almaqah-Tempels Bar'an, die ja als Thron der Königin von Saba bezeichnet werden, zu photographieren, wurde dies durch einen plötzlich aufkommenden Sandsturm vereitelt; auf dem Photo ist nur noch eine in Dunkel gehüllte, davoneilende schemenhafte Gestalt zu erkennen. Sie ist davon überzeugt, daß dadurch die Königin von Saba sie von ihrem Thron habe vertreiben wollen."

Is it possible? What's next?

The present material can manifestly not fit in Pirenne's straitjacket-like system. Would Garbini open his eyes to the epigraphical facts which he himself publishes he might see that the present collection is yet another denial of the theories which Pirenne keeps repeating to the obvious complacency and tacit approbation of those who are more interested in their own benefit than in the respect of the epigraphical material. But, since he still wants to hold to Pirenne's theories, he should at least out of respect for his reader have tried to square the circle by showing how and where the letter types of the present collection fit in Pirenne's house of cards instead of blindly throwing the irrealistic date of "about halfway through the 5th century B.C."

de Maigret himself shows a rather skeptical attitude toward the dates obtained by the carbon-14 tests as shown by his remark on the two dates from Hajar bin Ḫumeid, viz. "if these are accepted" (p. 19). Those two dates and any other of the kind are not comparable with any scheme based on speculation and preconceived theories; they give a well-grounded basis for further studies. de Maigret makes his remark after stating that "as is known, the G. van [sic!] Beek report was bitterly criticized in the past" (p. 19) with reference only to J. Pirenne's review in Revue archéologique, 1973, pp. 323-325.

First of all, the reader does not forget that that review is from the pen of an author who demonstrated without the shadow of a doubt in her volume on SA palaeography PiPISA that she bends, distorts and even eliminates whatever does not fit in her preconceived scheme which she applies in a most brutal manner to the information at hand and for the sake of which she would utilize anything at all. She does the same in her study of the excavations of Hajar bin Ḫumeid. This point is illustrated by her first remark on the archaeological section of her review. She writes that "On a voulu, paraît-il, faire vite pour obtenir une stratigraphie de poteries" (p. 323B). G. Van Beek himself correctly states that Hajar bin Ḫumeid was "an ideal site for

developing ... a pottery chronology for pre-Islamic South Arabia" (cf. Hajar bin Humeid, Baltimore, 1969, p. ix). But, since I was there during the whole campaign, I simply wish to state that Pirenne's repeat of that foul calumny renders her as vile and abject as the person (is it not she?) who uttered it for the first time.

Pirenne's biased and distorting approach to the information already appears in her paragraph on epigraphy; it reads as follows:

"On a trouvé sur ce site 22 inscriptions; or, 12 seulement sont figurées photographiquement; 9 (dont une avec mention de roi) sont inutilisables. A.Jamme n'indique même pas les planches, lorsqu'il y en a. Nous comprenons difficilement comment on peut encore publier des inscriptions sans reproductions, surtout dans des comptes rendus de fouilles" (pp. 323A-B).

First of all, the following data given by Pirenne must be corrected:

instead of "22 inscriptions," there are 23 sigla with 26 inscriptions;

instead of "12 seulement ... photographiquement," there are 14 + the tracing of the 4 of HI 2-27 in fig. 131 (p. 353) which could hardly be seen on a photo; and

instead of "9 (dont une avec mention de roi) sont inutilisables," there are 8, and none of them has the mention of a king; what she has in mind is HI 52 the two lines of which contain each a royal name with both the beginning and the end missing.

Among the texts not illustrated by a photo, are

H 2 a, a masonry mark;

H 2 d: ..]nb on a stone in a very bad state of preservation;

HI 3: the right end of three lines;

HI 13: eight letters belonging to the left part of a boustrophedon text;

HI 19: eight letters from a one-line text; etc.

Judging from the manner in which Pirenne applied her palaeographical scheme to the texts in PiPISA, her claim that those texts are "inutilisables" is pharisaical. Secondly, I was not in charge of the editing of the volume and, therefore, I bear no responsibility for either the missing cross-references in my text or the choice of the photos and figures which, with the obvious exception of fig. 131 and 132, I had not seen before the printing of the book.

Garbini identifies krbʾl/wtr of Ir 44 with his homonym of RÉS 3945, 3946, etc.Such an historical telescoping of about 300 years is disproved by palaeography, as already demonstrated in my volume on La paléographie..., pp. 126-132.

The two mukarribs and their respective fathers are most likely to be identified with their homonyms of the lines 4-6a of the stemma in JaMAR, X (1979),

pl. 2-4; they may be ascribed to the 8th century B.C.

3 - The mukarrib yt<ʿ>mr/byn.

a - First set of Garbini's remarks.

Beside the preceding remarks, Garbini talks about this mukarrib in his commentary on Ir 42 (p. 30), which reads as follows:

" This inscription of the *mukarrib* Yatha"amar appears less official than AQ/7 as it does not use the formula of the sacred hunt but limits itself to the secular or sporting aspect of the event. This is confirmed by the fact the inscription below, which in a certain sense completes the inscription itself (as AQ/8 does AQ/7), refers to the sovereign's wife, or favourite as she may have been. "

The first half of the commentary, which is repeated on p. 37, testifies to Garbini's obsession of things sacred because Ir 42 is as formal, as official as Ir 41 = AQ/7; and "the formula of the sacred hunt" is the produce of his imagination only. The only difference between Ir 41 and 42 is that the first commemorates a more elaborate hunt in two identified places and the second an ordinary hunt in one identified place; and who would state that a hunt without traps is more "secular or more sporting" than one with such devices?

The second half of the commentary is composed of two sections, viz. the main phrase itself and the parenthesis.

(1) The main sentence itself is doubly erroneous. Ir 43, "the inscription below" Ir 42, does not complete the latter "in a certain sense" or otherwise because it follows the normal pattern of such texts. What it does, as all other inscriptions engraved by the mukarribs' relatives and friends, is to inform on one of those who accompanied their master. Furthermore, Garbini should have explained how, in his opinion, Ir 43 could "confirm" his theory on the "secular, sporting aspect" of Ir 42 more, e.g., than Ir 59/2-3, 60 and Ga 97b by the mukarrib's brother, his son or an unknown person, respectively. All those four persons equally participate in the śʾk part of the hunt.

(2) The parenthesis of Garbini's statement, "as AQ/8 does AQ/7," is incomprehensible because AQ/8 = Ir 49, a text by krbʾl/wtr, is compared with AQ/7 = Ir 41, an inscription by yt<ʿ>mr/byn, and Ir 41 is more detailed than Ir 49! Therefore, from the viewpoint of the contents of the texts, it is just the opposite of what Garbini claims.

b - Another set of Garbini's remarks.

This second set of remarks may be found in his commentary on Ir 47(pp. 37-38).

(1) Ir 47, RÉS 4177 and Gl A 731.

After repeating a statement already made in p. 30 (cf. above, p. 37), Garbini compares Ir 47 with RÉS 4177 which he describes as "a monumental inscription carved on a large stone slab which must have been the lower part of a monument" (p. 37). The repetition of "monumental" and "monument" which describe respectively the letter type and the whole unit may lead the reader into error. It is, therefore, necessary to restate the measurements given by E. Glaser himself, viz. 1.5 m lang, 1.1 m breit, 0.55 m hoch" (cf. M. Höfner und N. Rhodokanakis in WZKM, 43 (1936), p. 213). It was a good-size stone block which should normally have supported a smaller stone with an illustration of the text itself.

Then Garbini claims that Gl A 731 "indicates that there was a temple of the god Athtar in that zone" (p. 37), viz. in the area of RÉS 4177. This is a gross misrepresentation of the text which speaks of the dedication by the son of a king of mwtb/ʿ(8)fwm/bmṣry(9)hw/bfrʿ (cf. M. Höfner in Sabäische Inschriften (Letzte Folge), Vienna, 1981, pp. 28-29). The double fact that the mwtb was located bmṣryhw at his [=the dedicator's] pool (Ar ṣariya "(water) to remain, stagnate long" and was paid for bfrʿ from first-fruits proves that mwtb could not possibly be a temple, but had to be a very small rural shrine.

After that, Garbini affirms that RÉS 4177

> " mentions neither the place where the hunt took place (Y 1 also remains silent on this point) nor the fact that the *qyf* was dedicated to Athtar ."

The first part of this statement is erroneous because any memorial of an unusual hunt would normally be set up where the hunt took place, viz. the hunting ground owned by ʿAttar (or His temple), and that hunting reserve of RÉS 4177 was at hlfy/nwmm the two passes of Nawmum (cf. above, p. 31). Furthermore, if the qyf of RÉS 4177 was not dedicated to ʿAttar it is obviously because the qyf did not bear any exclusively religious symbol. The same remark is also valid in Ir 47 because a qyf is not necessarily a religious monument (cf. already above, p. 30). However, Garbini's remarks were intended to serve as an introduction to his imaginary theory according to which (if I correctly understand his prose) the qyf "is much more likely to refer to those carved pillars which were erected on the occasion of sacred hunts, as Jacques Ryckmans has illustrated very well" with reference to RycCRA, pp. 280-298. Those pillars with the figures of ibex, snakes, ostriches, young women, geometrical designs, etc, are merely ornamental by representing things and animals known by the ordinary people and having as much to do with the so-called sacred/ritual hunt than the palm tree and the lion of CIH 72, the palm trees and the sphinxes of CIH 73, the ibex ornating so many of the mukarrib-period inscriptions, etc. It is with good reason that all the fana-

tics of the so-called sacred/ritual hunt carefully avoid not only mentioning all those representations ornating ordinary inscriptions but also explaining the presence of snakes which have never been the object of hunting parties, such as those described in the texts.

Let us take, for instance, the human example of that so-called sacred/ritual hunt on which J. Ryckmans so ineptly wrote in RycCRA (pp. 288-298, with a bad photo after p. 288 and a laughable facsimile on p. 290); e.g., in his "Conclusions" (p. 297), and like an old cracked record, Ryckmans repeats his obsession as follows: the "représentation humaine ... a une signification rituelle évidente, qui se dégage du costume élaboré, de la coiffure, de l'armement ... et des motifs: oryx et bouquetins, notamment, qui y sont directement associés" (my underlining). A reader unfamiliar with SA would never suspect that Ryckmans' preceding pompous diatribe deals with the same women as those represented in a photo published in a paper by R.Audouin and C. Robin in Dossiers. Histoire et archéologie (No. 122, décembre 1987, p. 54A) with this caption: "la jeune fille ... tient dans sa main droite un objet recourbé qui pourrait être une sorte de hache et dans sa gauche une longue hampe, parfois traitée comme une lance" and this short commentary on p. 55B: "la signification de ce décor est encore débattue. S'agit-il de divinités agraires ou de danseuses, peut-on établir un rapport avec la chasse rituelle?" Yet there is not a single clue even remotely suggesting the interpretation of those women as "divinités agraires" - and the authors do not give any - except the hypnosis of things religious. The solution of "danseuses" is also disproved by both their stance (they are standing, not dancing) and the objects surrounding them. Finally, the hypothesis of any connection with the "chasse rituelle" is a left-over of A.F.L. Beeston's 1948 decrepit fancy (cf. JaMAR, XV, pp. 80-82). Those women look like well-dressed shepherdesses with a long rod in the left hand, some kind of boomerang in the right and water jugs above their heads. The last two objects are seen by R.B. Serjeant as "curved sticks" and "pottery waterjars" (cf. South..., p. 104; cf. my remarks in JaCIASA, pp. 267-268). He does not speak of either "divinités" ("agraires" or not) or "danseuses" or "chasse rituelle" although his book attemps to defend the theory of that hunt. Serjeant's testimony is important to the extent that even to a defendant of the sacred/ritual hunt theory the interpretation of both Ryckmans and Audouin-Robin is based on preconceived ideas instead of on the objective analysis of the material. Finally, if Serjeant speaks of "the male figure" instead of women it is because he bases his study on A. Fakhry's two incomplete facsimiles (cf. An Archaeological Journey to Yemen (March-May, 1947), Cairo,

1952, Part I, p. 144) which he reproduces on p. 104.

The latest treatment of that decorative representation is found in CIASA, I, pp. 256-262, where J. Pirenne once more shows the best of her imaginative talents. After reproducing and studying both Fakhry's sketch and J. Ryckmans' scribbling, she decided that "avec, au-dessus de la tête, des gongs ou cymbales supendus, à leur main gauche le sistre, et dans la droite un instrument pour frapper sur les gongs, ce seraient des musiciennes" (p. 259). It surely takes Pirenne's nonsensical "folle du logis" to see "gongs" in a series of bulged, imbricated objects, a sistrum in a long stick or spear, or to think that those people used a spear "pour frapper sur les gongs."

(2) His "stages of the hunt."

Garbini's description of what he calls "the stages of the hunt" (pp.37-38) is a nice exhibit of delirium.

(a) "Participation of all the court."

"The hunt itself with the participation of all the court" (p. 37) is given as the first element of his whole scenario. It is impossible to understand how Garbini dares to write such a statement after affirming ten pages before (p. 27) that "it may not be merely by chance that only four people of those who took part in the hunt declare themselves to be companions of Yataʿʾamar [in Ir 44, 45, 64, Ga 97b] as against the eight [in Ir 50, 51, 52, 53, 54, 56b, 57, 58] and the son [in Ir 55b] of Karibʾil" (p. 27). What can likely be the meaning and purpose of such a cryptic statement? At any rate, according to his tally, there would have been only "four" and "eight" "companions" of ytʿʾmr/byn and krbʾl/wtr, respectively. These are indeed impressive numbers of persons not representing the court but, as Garbini puts it, indicating "the participation of all the court" (my underlining). Furthermore, in his list of ytʿʾmr/byn's "companions," Garbini eliminates from the list of each mukarrib both Ir 59 and 60, which refer, respectively, to two separate hunts and one common hunt by the two mukarribs. He also eliminates the authors of Ir 43 and 46b, although these two inscriptions are by ytʿʾmr/byn's brother ʾbkrb and son dmrkrb, respectively. It is illogical of Garbini to eliminate those two texts since he lists "AQ /24" = Ir 55b by yqhmlk, krbʾl/wtr's son, among the "companions" of the mukarrib.

The immediate purpose of the following schema is to present data on the hunting parties sponsored by the two mukarribs based on the place names recorded in each inscription involved. To that schema are also added two more columns containing the verbs and the ductus of the texts so as to make immediately available to the reader the information showing how distorting many of Garbini's statements really are.

Places	Sponsors	Texts	Verbs	Participants	Ductus
unspecified	the two rulers	Ir 60	ś ʾk	dmrkrb	sen
	krbʾl/wtr	Ir 56b	ś ʾk	mʿdkrb	
		Ir 59/3-4	ś ʾk	ʾbkrb	bou
unavailable	ytʿʾmr/byn	Ir 64	ś ʾk	dr...	bou
dnm	ytʿʾmr/byn	Ir 59/2-3	ś ʾk	ʾbkrb	sen
		Ir 44	ś ʾk	zwrkrb	bou
ʾrydy	ytʿʾmr/byn	Ir 42 X	ṣd	—	sen
	krbʾl/wtr	Ir 54	ś ʾk	gbḫm	bou
		Ir 58	—	ʾdbʿly	sen
ʾrydy/wdnm	ytʿʾmr/byn	Ir 43	ś ʾkt	gṯmt	sen
		Ga 97b	ś ʾk	?	sen
	krbʾl/wtr	Ir 49 X	ṣd	—	sen
dnm/wʾrydy	ytʿʾmr/byn	Ir 41 X	ṣd	—	bou
		Ir 45	ś ʾk	dmryfʿ	sen
		Ir 46b	—	two women	sen
krbʾl/wtr		Ir 50	ś ʾk	smhʾmr	sen
		Ir 51	ś ʾk	brhmw	bou
		Ir 52b	[ś]ʾk[/wṣyd]	ʿmsfq	bou
		Ir 53	ś ʾk/wṣyd		bou
		Ir 55b	ś ʾk/wṣyd	yqhmlk	sen
		Ir 57	ś ʾk	hlkm	sen

bou = boustrophedon; sen = senestrograde. - The three texts engraved by the two mukarribs, Ir 41, 42, 49, are marked with X. - Ir 60 is characterized by the preposition bʿm introducing the name of each mukarrib and by a single tally at the end. This feature indicates that there was a single hunting party when compared with Ir 59 where the distinction of the two hunts is marked by the repetition of ʾlt/ʿm before the name of each mukarrib followed by a separate tally for each hunt. Ir 60 remains the only text mentioning a hunt sponsored by the two mukarribs.

In the following presentation of the hunts held separately by each mukarrib, the two expressions dnm/wʾrydy and ʾrydy/wdnm are considered as identical, viz. they refer to the very same hunt. However, when a place name is followed by

more than one reference, there is no way of knowing whether there actually was one hunt only or more than one, as is the case of Ir 44 and 59/2-3. It is also assumed that no hunt was held at a place other than drm and ʾrydy.

yt[ʿ]ʾmr/byn led at least three hunts of his own, viz.

one at drm in Ir 44 and 59/2-3;

one at ʾrydy in Ir 42; and

one at drm/wʾrydy in Ir 41, 45, 46b OR ʾrydy/wdnm in Ir 43 and Ga 97b.

There is no way of knowing to which of the three preceding hunts Ir 64 belongs because the place name is not available.

Ir 47 cannot be taken into consideration because of the difference in phrasaeology.

krbʾl/wtr led at least three hunts of his own, viz.

one at ʾrydy in Ir 54 and 58; and

two at drm/wʾrydy in Ir 52b and 53, by the same author but with two different tallies.

There is no way of knowing to which of the two preceding hunts any of the five following texts refers, viz.

at ʾrydy/wdnm in Ir 49; and

at drm/wʾrydy in Ir 50, 51, 55b and 57.

There is no way of knowing whether any of the two following texts refers to any of the three above-listed hunts or to yet a different one at drm, viz. Ir 56b and 59/3-4 at an unspecified place each.

The following considerations are in order.

a - The preceding schematization of the data lists at least four hunts per mukarrib. If it would be irrealistic to take each text as referring to a unique hunt, it seems almost certain that each mukarrib sponsored more than four hunting parties. But the exact number will never be known because the inscriptions were not intended to provide that kind of information.

b - Even were all the hunts put together as if making a single party, which is impossible, it would still be ridiculous to call "the participation of all the court" the nine and sixteen persons explicitely mentioned in yt[ʿ]ʾmr/byn's and krbʾl/wtr's groups. Note however that ʾbkrb and dmrkrb, yt[ʿ]ʾmr/byn's brother and son, are counted twice since they went hunting with the two mukarribs.

c - The mere multiplicity of at least four hunts per mukarrib makes it also ridiculous to even think that such a hunt was a part of any mukarrib's "investiture."

d - The mere multiplicity of those hunts suggests that they were small parties organized at will by the mukarribs.

e - Only a fanatic obsession of things religious could see in those texts some allusion or indication of any kind of sacred/ritual hunt.

The second "stage" of Garbini's scenario is "the erection of a gyf." (b) "The erection of a gyf in honour of the divinities who had watched over the hunt and ensured its success" (p. 37). But, as explained above (p. 30), gyd/ʿttr/wkrm are two place names, viz. ʿAttar's hunting ground and the place where those pits or traps were dug. When those words are governed by the noun gyf, they mean their representation by their respective symbol. Furthermore, "the erection of a gyf" specified by Garbini is attested once only in ytʿʾmr/byn's Ir 47, which does NOT belong to the al-ʿAqûl collection, and not once in any of krbʾl/wtr's texts. In RÉS 4177, the same mukarrib ytʿʾmr/byn also erected an ordinary gyf because it is not indicated as being ritual or sacred.

(c) "The monument was erected in a sanctuary of Athtar which was evidently linked in some way to king Yaṯaʿʾamar Bayyin" (p. 37): this is the third "stage" of Garbini's production. But the idea of "a sanctuary of Athtar" comes from his misunderstanding of Gl A 731 (cf. above, p. 38), which is also foreign to the present collection. Furthermore, Garbini qualifies erroneously ytʿʾmr/byn with the title of "king" although his only title is repeatedly attested as mkrb (Ir 41/2, 42/2, 47/1 and RÉS 4177/2).

(d) "The writing of an official inscription, located near the capital to commemorate the erection of the gyf on the occasion of the hunt" is the fourth "stage" of Garbini's setting. But, as already pointed out above (p. 42), Ir 47 in which ytʿʾmr/byn narrates that he gf/gyf/ʿttr/wkrm (cf. above, p. 28) does not belong to the collection of the rock texts from the al-ʿAqûl area; and neither of ytʿʾmr/byn's two official records, Ir 41 and 42, nor krbʾl/wtr's single official record, Ir 49, contains the words gf and gyf/ʿttr. Two conclusions are immediately obvious.

(1)- Ir 47 refers to a very special hunt and this is confirmed by the fact that ytʿʾmr/byn had it recorded in a special inscription engraved on an isolated stone, not on the rocky panels around al-ʿAqûl. Ir 47 remains without any parallel in the al-ʿAqûl collection.

(2)- Ir 41 and 47 cannot refer to the same hunt because it would be inconceivable that the first would refer to the hunt of the second without any mention of the latter's essential element, the erection of a gyf, which was the apparent reason for its engraving.

(e) Garbini's omissions.

Garbini apparently forgets three important elements of that hunt according to his own dreamy scenario, viz.

(1) "A royal hunt seems to have lasted for some days" (p. 21);

(2) The "foreign guests, writing in North-Arabic, enjoy the place of honour immediately after the mukarribs" (p. 22); and

(3) "The royal hunt, which in old times was a prerogative of the mukarribs, also involved the sacrifice of animals" (p. 22).

Of course, it is quite normal for Garbini who retains the existence of one hunt only recorded in all the al-ʿAqûl inscriptions to declare it "to have lasted for some days." Here again, the adjective "royal" (cf. also the following (3)) is erroneously used in a mukarrib context (cf. above, p. 43). Furthermore, the only "North-Arabic" text found by Garbini is Ga 95 which is engraved in the only space available between Ir 49 and 62; it even follows the curves of the two texts. The left-over place between two texts is a rather niggard "place of honour"! Finally, the idea of "sacrifices" comes from his blundering analysis of Ir 61 (cf. below, p. 73).

(f) Garbini's conclusion.

Garbini's conclusion of his imaginary staging involves more produces of his fertile fancy.

(1) He speaks of "an event of exceptional importance, even involving the erection of a sacred monumental complex. The sacred hunt can therefore be seen as representing for the mukarribs of Saba an essential part of their investiture as sovereigns" (pp. 37-38). I wonder what kind of "event of exceptional importance" it was for yt[ʿ]mr/byn to have his name engraved isolatedly twice on the rock in Ir 55a and 56a. Furthermore, the mention of "the erection of a sacred monumental complex" is inexplicable unless as a reminder of both the fantastic story of the so-called temple in Gl A 731 (cf. above, p. 38) and his erroneous inclusion of Ir 47 in the rocks texts of al-ʿAqûl area (cf. above, p. 43). Moreover, what kind of "essential part of their investiture" might have taken place and/or what kind of "investiture" would there have been since, as already pointed out above (p. 42), yt[ʿ]mr/byn and krb[ʾ]l/wtr made each at least four hunts and, therefore, each of them should have received the "investiture" at least four times! Of course, Garbini might suggest that each anniversary of the investiture could have been celebrated in a special manner. But, since there is no published text mentioning the investiture itself of any ruler, it would be another wishful thinking to expect any indication of a possible anniversary celebration. Besides, no investiture is ever repeated, whether or not its anniversary is an occasion for celebration. Finally, it is regrettable that

Garbini did not elaborate on his remark (p. 26), viz. "It may not be merely by chance" that the mukarribs' "companions" were so few (cf. already above, p. 40). The only conclusion he should have inferred from it is the foolishness of his claim that the hunt was "an essential part of their [= the mukarribs'] investiture as sovereigns." Such an "investiture" must have been another wonder of the ancient world since only twelve persons, according to himself, signed up as participants of at least eight ceremonies of "investiture"!!!

(2) Garbini then adds a very important precision, viz. "the hunt of Yata[ʿ]amar probably took place when he assumed the full duties of king on the death of Yada[ʿ]-[ʾ]il" (p. 38). There comes again the mention of "king" (cf. above, p. 44). Furthermore, the name of "Yada[ʿ][ʾ]il" apparently comes from Ir 48/1 (cf. also al-Iryânî's stemma above, p. 22), where yd[ʿ][ʾ]l and yt[ʿ][ʾ]mr are given as co-regents. But, if my assumption on the origin of yd[ʿ][ʾ]l is correct, how can a co-regent succeed his own co-regent? Furthermore, Garbini's expression "the full duties" is ambiguous, and his statement is contradicted by the fact that yt[ʿ][ʾ]mr/byn and krb[ʾ]l/wtr were, even for him (cf. below), co-regents. Therefore, how can he speak of "the full duties" of yt[ʿ][ʾ]mr/byn since the latter's texts were engraved during that very co-regency? In Ir 48 as well as in the rocky texts from the al-ʿAqûl area, yt[ʿ][ʾ]mr/byn's duties were shared with his co-regent; they were not "full."

(3) Garbini closes (p. 38) his commentary on Ir 47 with the repeat of the preceding conclusion but not without blurring even more the epigraphic material. He speaks of "inscriptions referring to sacred hunts for only two *mukarribs* (Yatha"amar and, with only one inscription, Karib'il Watar: RES 3946)."

This statement means that, according to Garbini, all the al-ʿAqûl inscriptions + Ir 47 (from one unknown place) + RÉS 4177 + Gl A 731 refer to only one "sacred hunt" of yt[ʿ][ʾ]mr/byn, while that of krb[ʾ]l/wtr is recorded in RÉS 3946. Garbini's affirmation is nothing but the initial statement of the commentary, viz. Ir 47,47 and RÉS 4177 "refer to a sacred hunt carried out by Yatha[ʿ][ʾ]amar Bayyin I" (p. 37) blown up out of proportion by the telescoping power thanks to which all items seen in the perspective of one preconceived idea are fused into a single reality and in the process all differences are eliminated as, e.g., in the present case, krb[ʾ]l/wtr's eleven formal records, including Ir 49 which the mukarrib himself engraved. Such an exhilarating statement could evidently not be curtailed and should instead be given full rein: Garbini took advantage of it to let his record unwind and repeat once more that "the sacred hunt was an altogether exceptional event... and probably connected with the coronation of a king" (p. 38). Furthermore, the

mention of a hunt in RÉS 3946/7 is found inside a long list of territorial acquisitions by either conquest or purchase by the mkrb krbʾl/wtr and could hardly be taken as "an altogether exceptional event" or as "probably connected with the coronation of a king." Moreover, Garbini's comment on Ir 41 states that "in any case, Krwm is subordinate to Athtar, as is shown by the inscription RES 3946 (l. 7) which mentions ṣd krwm but offerings made only to Athtar" (p. 26). Garbini's interpretation might be taken into consideration were the pericope hᶜ/mhyᶜ/lqṣ/ᶜttr (l.7) he built the wide wall of Laqiṣ for ʿAttar, linked to ṣd/ṣyd/krwm by the conjunction w and, which would have placed the two pericopes in the dependence of the same conjunction ywm. Instead,as ywm introduces each pericope, it is certain that each pericope describes an event distinct from the other. Furthermore, since we are not even sure that the list of events follows the chronological order of their succession, it is foolish to ascribe the two ywm-pericopes to the very same period and even more to link them together as parts of the same activity.

4 - The mukarrib krbʾl/wtr.

Garbini's commentary on Ir 49 (p. 27), the only inscription by this mukarrib, is another gem of his literary production.

a - Garbini's initial statement.

The initial statement of Garbini reads as follows:

"This inscription is particularly important as it provides the first evidence of the contemporaneity of two *mukarribs* and thus of the practice of co-regency dating right back to ancient times"

The text Ir 49 being the normal record of a hunt does not prove the co-regency any more than ytᶜʾmr/byn's Ir 41 where Garbini does not make the same remark although he publishes it in the preceding page (p. 26). What Garbini had probably in mind is this: since Ir 49 is engraved immediately below Ir 41, the two texts were engraved during the same period and this in turn makes the two mukarribs co-regents. Such a reasoning is unacceptable because he does not prove that the proximity was intentional. Garbini further claims "a de facto predominance of Karibʾil over the possibly milder Yathaᶜʾamar" (p. 27). From the viewpoint of the inscriptions, even the two texts Ir 59 and 44, although more explicite than Ir 41 and 49, do not solve the problem of the co-regency. In Ir 59, a brother of ytᶜʾmr/byn takes part in a hunt with his brother and in another hunt with krbʾl/wtr. These two hunts are different since the bag of the kill is different in each case. The most conspicuous example is Ir 44, text of ẓwrkrb, a nephew of krbʾl/wtr, who speaks of his beating up with ytᶜʾmr/byn, with a very low tally, 50; only Ir 43/4 and 64/2 show lower tallies with 20 and 25, respectively. Yet, the author splashes his text composed of large letters, viz. 18.5

x 3 (on the right) cm, almost all the way across the whole panel of pl. 46, and his tally, 1.8 x 0.7 cm, is 2.2 cm below ᶜ of ytᶜʾmr/byn. The two adjacent texts by two followers of krbʾl/wtr, viz. ẓbḥm of Ir 54 and ᶜmšq of Ir 53, respectively, above and below Ir 44, look like dwarfs, e.g., ʾšʾk measures 3 x 1.7 cm in Ir 44, but only 1.8 x 0.8 and 1.6 x 0.9 cm in Ir 54 and 59, respectively. And yet,the tally of each of these two texts, 1000, is 20 times that of Ir 44. For my tracing of Ir 44, 53 and 54, cf. above, p. 4. The information given by these two texts makes it certain that the reigns of the two mukarribs were close to each other and that ytᶜʾmr/byn was the first, but it still does not prove the co-regency of the two mukarribs. Furthermore, Garbini's above-quoted comical affirmation on ytᶜʾmr/byn's personality is a mere imaginary concoction. The identification of the texts in pl. 38, the most complete general photo containing pl. 41, 42, 44, 45, 46 and 47a indicates that the best places are occupied by the seven texts written by ytᶜʾmr/byn and his followers, viz. Ir 41, 42, 43, 44, 46b, 59 and 60, while krbʾl/wtr and his adherents had to contend themselves with leftover, second-grade spots to write their five inscriptions, viz. Ir 49, 53, 54, 57 and 58, and this confirms either the anteriority or the leadership of ytᶜʾmr/byn. That ytᶜʾmr/byn was "possibly milder," not even Garbini knows that unless a magical hoopoe whispered it in his ear, but the mukarrib surely was efficient in choosing the best locations for his inscriptions as shown by the setup of his texts on the rocky panel. A good example is Ir 49: the only text engraved by krbʾl/wtr is wedged below ytᶜʾmr/byn's Ir 41, and its engraving had to make use of the leftover space at the expense of the regularity of the letters, of the straightness of the lines, and of the regular place of the double symbol. The actual engraving of these two texts, Ir 49 and 41,proves the co-regency of the two mukarribs with the indisputable primacy of ytᶜʾmr/byn. On the one hand, had krbʾl/wtr been the leader, he would have had the prime choice for his only text and would not have been contended with a second-grade spot, a leftover, to engrave it. If he did engraved it, it most certainly is because he had no choice in consequence of his dependency upon ytᶜʾmr/byn. On the other hand, had he been the sole mukarrib in power, there were many other places where he could have made his hunt and, therefore, engraved his text. Even if because of an unknown reason he wanted to have his hunt in the same area as that of former ytᶜʾmr/byn, he still had other places, as shown on pl. 12, where he could have engraved his text freely and normally without being forced to making the best of a small crammed spot left over by ytᶜʾmr/byn.

That co-regency and ytᶜʾmr/byn's primacy are also proven quite clearly by

Ir 60 which records a hunt sponsored by the two mukarribs (cf. already above, p. 41) and mentions yt^c>mr/byn first. Finally, the so-called "de facto predominance of Karib>il" is further exemplified but the wrong way by the simple fact that his name is even less frequently attested in the texts than yt^c>mr/byn's, viz. 13 against 15. This surely is a strange way to show his "de facto predominance."

b - Garbini's deceitful summary.

The preceding remarks are best followed by Garbini's piece of deceit on the question of mukarrib co-regency. He refers his above-quoted (p. 46) statement to

> "the prudent views of J. Ryckmans, *L'institution monarchique en Arabie méridionale avant l'Islam*, Louvain 1951, pp. 96–97 and J. Pirenne, *Paléographie des inscriptions sud-arabes*, Brussel 1956, pp. 117–18; for the existence of the co-regency, see G. Garbini, in *Oriens Antiquus*, 12, 1973, pp. 160–62). The recognition of this co-regency destroys the edifice on which the «long» chronology is constructed" (p. 27).

In fact Ryckmans' "prudent views" actually means Ryckmans' rejection of the mukarrib co-regency and his approval of their successive reigns only. Garbini's "and" between Ryckmans's work and Pirenne's PiPISA cannot but indicate that the two works are considered as presenting the same line of thoughts. Yet PiPISA (pp. 116–118) takes the stand opposite to Ryckmans' as already indicated clearly by the title of the second half of her presentation, viz. "Règnes parallèles" (p. 117). As far as I am concerned, a text, such as CIH 375 = Ja 550 (cf. JaSIMB, pp. 9–10), which is not even mentioned in Garbini's referred-to paper, has always been understood as a proof of that co-regency, viz. tb^ckrb, the administrator of yd^c>l/byn, ykrbmlk/wtr and yt^c>mr/byn, lists these three mukarribs to whom krb>l/wtr is added, in his final invocation; and such an invocation always mentions living rulers. Finally, Garbini's 1973 paper does not introduce new ideas; it does nothing but develop Pirenne's thesis.

c - The use of the verbs ṣd and ś>k.

Garbini notes that the two mukarribs "use the verb ṣd,'to hunt', whereas the others use s̱>k" (p. 27). Garbini's irresponsability is manifest: Ir 53 and 55b are by a servant and the son of krb>l/wtr, respectively, and each of those two men uses the formula ś>k/wṣyd (cf. already above, p. 41). In his commentary on either text (pp. 32 and 33), Garbini fails to refer to his above-quoted affirmation. However, in his commentary on Ir 53 (p. 32), he declares that the use of the verb ṣyd in those texts "is an exception" and, therefore, that the authors of those inscriptions "held a particular position in the court." Garbini has just invented the "lapalissade" of the XXth century, viz. "it must be admitted" that the son of krb>l/wtr, author of Ir 55b, "held a particular position in the court"!!! In his commentary on Ir 55b (p. 33),

there is no reference to the preceding statement. For the two verbs ś>k and ṣ(y)d, cf. below, pp. 53–57.

d - yt^c>mr/byn and "the formula of the sacred hunt."

According to Garbini, "only Yatha^c>amar, whose position was superior to that of Karib>il, uses the formula of the sacred hunt" (p. 27). On yt^c>mr/byn's superiority Garbini contradicts himself when he speaks a mere eleven lines down of the same p. 27, of "a de facto predominance of Karib>il" (cf. already above, pp. 46–47). Furthermore, the crucial question is to know what Garbini understands by "the formula of the sacred hunt." Unfortunately, he does not explain himself. Logically - but Garbini contradicts himself so often that the reader does not know whether or not he is logical in this case - he should consider that formula as at least mentioning "the erection of a ṣyf" according to his p. 37, thus, ṣf/ṣyf. But then, n o n e of the two mukarribs' texts from al-^cAqûl contains that expression; it is found only in Ir 47 which comes from some unknown place and was seen in Yalâ (cf. already above, p. 43). Therefore, if he is logical (again?) with his own theory, n o n e of a l l the al-^cAqûl inscriptions actually refers to "the sacred hunt," and if he speaks of it, as he does, it is nothing but another illogical, whimsical speculation.

e - krb>l/wtr's so-called "impatience."

Garbini continues his parody of the epigraphical material by stating this:

> " It is of interest to mention here certain evidence in the al-'Aql inscriptions of impatience on the part of Karib'il with his subordinate position and his desire to distinguish himself. He alone inverts the words *'rydy wdnm* and, unlike Yatha''amar, uses boustrophedon script. His royal symbols (ḍ ḥ) are larger than those of Yatha''amar and his inscription, unlike that of Yatha''amar, is delimited laterally. These are small things, but they still reveal the personality of the ruler " (p. 27).

Note again the use of the adjective "royal" when dealing with mukarribs. Furthermore, contrary to Garbini's statement on "that of Yatha^c>amar," the mukarrib yt^c>mr/byn's second text, Ir 42, is as "delimited laterally" as Ir 49. Moreover, Garbini's presentation of the details listed above completely distorts the picture offered by the texts (cf. already above, p. 41) and, therefore, his conclusion drawn from them is simply preposterous.

(1) Garbini's statement on krb>l/wtr being the one who "alone inverts" the words >rydy and dnm is erroneous. The same order of the names is also found in Ir 43 by ġḥmt, a maidservant of yt^c>mr/byn, as well as in Ga 97b (cf. above, p. 41). Most importantly, the order of the two names is

NOT reversed in all the inscriptions ((there is no exception)) written by the members of krbʾl/wtr's family and by two of his followers (cf. above, p. 41), viz.

Ir 55b, by yḥmlk, his son;

Ir 57, by hlkm, his brother;

Ir 50, by smhʾmr, his nephew and the son of the preceding hlkm;

Ir 51, by brhmw, a follower; and

Ir 52 and 53, by ʿmšfq, a follower and the son of the preceding brhmw.

If, as Garbini claims, krbʾl/wtr intended to "reveal his personality" and "to distinguish himself" by reversing the order of the two place names in his Ir 49, he was flailing the air because some of ytʿʾmr/byn's entourage had already done it (cf. above, p. 41), and his show was a complete failure because nobody in his entourage followed suit.

(2) The preceding reflections also apply to Garbini's stand on the ductus of the texts (cf. already above, p. 41). Contrary to his statement on "unlike Yathaʿʾamar," this mukarrib uses the boustrophedon in his own Ir 42; and his brother, ʾbkrb, does the same in Ir 59. Furthermore, if krbʾl/wtr's boustrophedon is adopted by his brother mʿdkrb in Ir 56b, it is not by his two other brothers, lḥyʿtt in Ir 63 and hlkm in Ir 57, as well as by his own son, yḥmlk in Ir 55b.

The freedom of choice between the two ductus, already proven by the preceding examples, is further illustrated by the family of the above-mentioned hlkm, viz. his sons ʾbdʿly and ẓwrkrb, as their father, use the senestrograde ductus in Ir 58 and 44, respectively, while his other son smhʾmr uses the boustrophedon in Ir 50.

The same freedom of choice is also attested in the family of a follower of krbʾl/wtr, brhmw, who uses the boustrophedon in Ir 51, but his son ʿmšfq uses both ductus, viz. the senestrograde in Ir 53 and the boustrophedon in Ir 52b.

Here again, the texts hardly prove the effectiveness of krbʾl/wtr's "desire to distinguish himself," as Garbini puts it, because, here again, it was an overwhelming failure.

(3) As to the additions to the texts, viz. the double symbol and the vertical lines, Garbini here again speaks out of his head. His description of the double symbol to the right of Ir 49 by krbʾl/wtr is not "larger" than that of both Ir 41 and 42 by ytʿʾmr/byn; it is slenderer and taller than the two others. The shape of that symbol in Ir 49 was imposed upon the engraver by the roughness of the rock surface: he compensated the lack of space to make it wider by making it taller. Furthermore, the two vertical lines in Ir 49 are manifestly used to make the text stand out better as a separate unit. It seems ridiculous to labor such small differences so as to see in them, as Garbini does, an indication of the mukarrib's "desire to distinguish himself." Moreover, Garbini's presentation of the vertical lines bordering the lateral sides of Ir 49 is also deceitful because those lines also exist in ytʿʾmr/byn's Ir 42, viz. two sections instead of a single line bordering the left side of the inscription. On the right side, the vertical line is the prolongation of the right vertical side of d, as in Ir 49.

The final redaction of the mukarribs' inscriptions and their engraving were no doubt entrusted to two professionals of the court, the writer and the engraver, respectively; "de minimis non curat praetor." Those two specialists used at discretion the different styles and ductus of their own period.

f - Ir 44, 55, 56, 59 and 60.

Garbini ends up his concoctions on the "companions" of the mukarribs by noting that

> "The presence of this son in a special position (see the comment to AQ/24) affords a glimpse of a de facto predominance of Karibʾil over the possibly milder Yathaʿʾamar" (p. 27).

"This son" is krbʾl/wtr's, yḥmlk of Ir 55b. For the expression "possibly milder Yathaʿʾamar" and the "de facto predominance of Karibʾil," cf. already above, pp. 46-49. Furthermore, contrary to Garbini's argumentation, the mention of Ir 55 = AQ/24 demands that of Ir 44, 56, 59 and 60 because, according to Garbini's interpretation, these five sigla would testify to the same social crossing over.

(1) The texts of the five sigla.

Ir 44: Garbini, p. 31; al-Iryânî, Ar p. 59; immediately below Ir 54; pl. 13, center of the central group; pl. 38, center of the penult right group of the right series; the last letter of 1.2 in pl. 45, extreme right center; pl. 46, second from the top; cf. my tracing above, p. 4; for some remarks, cf. above, pp. 6 and 24; cf. summary above, p. 41.

ʾlt/śʾk/ẓwrkrb/bn/hlkm	1	These has beaten up Ẓawwarkarib, son of Halkum,
dnm/ʿm/ytʿʾmr/byn	2	at Danam with Yataʿʾamar:
9	3	50.

In 1.1, the letters rb are distorted; the second ʿ of 1.2 is clear; the vertical line registered at the end of the text by al-Iryânî belongs to Ir 42. The number of the tally belonging to this text is mentioned with Ir 53 by al-Iryânî (Ar p. 67). Garbini states that "the intrusion of AQ/20 [= Ir 53] ... makes the original spacing of the epigraph less evident." This statement does not make any sense. The difference in letter height is so great that even at first sight the two texts are clearly defined. The matter would be different were Ir 53/1 engraved between the two lines

of Ir 44. But such is not the case. For ṯwrkrb, cf. ṯwr⁾mr of Ir 66a (cf. below).

Ir 55: Garbini, p. 33; al-Iryânî, Ar p. 68; pl. 47c; for some remarks, cf. above, pp. 6 and 9–10; for the identification of yqhmlk, cf. above, pp. 17 and 24; for the question of NA, cf. above, pp. 32–33; for the summary, cf. above, p. 41; the location of the text remains unknown, cf. above, p. 8.

a	yṯ⁽⁾mr/byn	a	Yaṯa⁽⁾amar Bayyin.
b	⁾lt/ś⁾k/wgyd/yqhmlk	b 1	These has beaten up and hunted Yaquhmalik,
	bn/krb⁾l/wtr/dnm/w⁾rydy	2	son of Karib⁾il Watar, at Danam and ⁾Arayday
	b⁽m/⁾bhw[/]krb⁾l/○𐡛𐡛𐡛	3	with his father Karib⁾il: 460.

Garbini's transliteration includes a nonexistent word divider after ⁾bhw; al-Iryânî does the same but puts it in stippling and he correctly marks the difference of letter height between the two texts.

Ir 56: Garbini, p. 30; al-Iryânî, Ar p. 69; pl. 43; there is hardly any space between the two texts; for some remarks, cf. above, pp. 6 and 9–10; for the identification of m⁽dkrb, cf. above, pp. 16 and 24; the location of the text remains unknown (cf. above, p. 8); cf. the summary above, p. 41.

	a	/yṯ⁽⁾mr/byn[/]	
←	b	m⁽dkrb/bn/dmr⁽[ly]	1
→		[w⁾](l)t/ś⁾k/⁽m/krb[⁾l]	2
←		𐡛𐡛	3

a	Yaṯa⁽⁾amar Bayyin.	
b 1	Ma⁽adkarib, son of Damar⁽a[lay:]	
2	[and th](e)se he has beaten up with Karib[⁾il:]	
3	50	

Text a: both Garbini and al-Iryânî fail to note that another word divider was also at the end of the line. al-Iryânî does not register the vertical line at the beginning of the line and, here again, he marks the difference of letter height between the two inscriptions.

Text b: all destroyed letters are given as existing by al-Iryânî, but he correctly restores w before ⁾ in l.2; cf. also Ir 56 b/2 and 59/2; Garbini fails to restore the conjunction. There are indeed two letters missing at the left-hand end of each line. al-Iryânî does not register any number, but Garbini reads "60." His "○" = ten is a circular feature of the rock located to the left of the lower half of 𐡛 and upon which the left marker is engraved. The number is found between the two well-known markers isolating the numbers.

Ir 59: Garbini, p. 30; al-Iryânî, Ar p. 71; pl. 38, extreme left section of the right series; pl. 44; for some remarks, cf. above, p. 6; for the identification of ⁾bkrb, cf. above, pp. 15–16 and 24; for the question of NA, cf. above, pp. 32–33; for the summary, cf. above, p. 41.

←	1	⁾bkrb/bn/smh⁽ly
→	2	w⁾lt/ś⁾k/⁽m/yṯ⁽⁾mr
←	3	dnm/𐡛𐡛𐡛/w⁾lt/⁽m/krb⁾l/
←	4	/𐡛𐡛/

1	⁾Abkarib, son of Sumhu⁽alay:
2	and these he has beaten up with Yaṯa⁽⁾amar
3	at Danum: 300; and these with Karib⁾il:
4	200.

al-Iryânî forgets the vertical line on the left of the number. Both al-Iryânî and Garbini fail to note that l. 4 is senestrograde as l. 3. Furthermore, Garbini notes that the letter ś "has a particular form" (cf. above, p. 33). This peculiarity is described as "a rounded shape" in the same p. 30 on Ir 43, without any cross-reference. In p. 34 on Ir 45, however, Garbini repeats the same description and, finally, informs his reader that the same feature is also found in Ir 59 and 43. This form of ś indicates a different way of tracing ś, viz. instead of intertwining two angular r's back to back, the engraver first traced an almost circular ellipsis to which he appended four short, aslant straight bars, two at the bottom and two at the top. Finally, Garbini fails to point out that ś of Ir 64/1 on which he makes no remark on p. 34, shows a combination of curve and broken lines, viz.) and < ; thus ✕.

Ir 60: Garbini, pp. 29–30; al-Iryânî, Ar p. 72; pl. 14, left half; below and to right of the panel of pl. 41; pl. 38, center; pl. 42; for some remarks, cf. above, p. 7; for the identification of dmrkrb, cf. above, pp. 16 and 24; cf. summary above, p. 41.

⁾lt/ś⁾k/dmrkrb[/]bn	1	These has beaten up Damarkarib, son of
yṯ⁽⁾mr/b⁽m/ ˌyˌṯ⁽⁾mrˌ/ ˌbˌyˌ[n]/	2	Yaṯa⁽⁾amar, with Yaṯa⁽⁾amar Bayyi[n]
wˌb⁽m/krb⁾l⌜/⌝ˌwtr⌝/𐡛/	3	and with Karib⁾il Watar: 50.

L.1: al-Iryânî does not record the vertical line to the right of the beginning of the text. The word divider before bn is not visible although there is ample space for it. – L.2: ⁽ of the second yṯ⁽⁾mr is clearly traced (contrary to Garbini) and is duly recorded by al-Iryânî. The last vertical bar is understood as "m" by al-Iryânî, but as "n̊" by Garbini. The n of byn, non visible on the chalked photo, is located between ˌyˌ and the last word divider. – L.3: the last six signs are still clear enough to be read.

(2) The meaning of the verbs ś⁾k and ṣ(y)d.

The isolated verb ś⁾k is attested in eleven texts, e.g., Ir 45/1, 50/1, 51/1; and also once the feminine form ś⁾kt in Ir 43/1; the formula ś⁾k/

wqyd is found in Ir (52 b/1), 53/1 and 55 b/1.

(a) al-Iryânî's translations of the verb ś'k are best presented as they are found in his paper, viz.

ś'k in Ir 54/1..... صاد

ws'kt in Ir 46 b/1..... وساكت

'lt/ś'k in Ir 44/1, 45/1..... هذا ما صاد به

Ir 50/1, 51/1, 57/1, 60/1, 64/1... هذا ما صادہ

Ir 52/1..... الذی صادہ

w'lt/ś'k in Ir 56 b/2..... و- ما صادہ

Ir 59/2..... وهذا ما صادہ

'lt/ś'kt in Ir 43/1..... هذا ما صادت

'lt/ś'k/wqyd in Ir 53/1, 55 b/1..... هذا ما ساك وصاد

In three texts - Ir 46/1 must be deleted because it is based on an erroneous reading - ś'k is simply transliterated and, thus, untranslated although the two verbs ś'k and syd are considered synonymous in all remaining texts. In his presentation of ś'k in pp. 41-42, al-Iryânî first states that ś'k means "has hunted" without any etymological support and apparently because of the fallacious assumption that ś'k must be somehow synonymous of syd since they are united in the same formula. His second point is to suggest that "the general meaning of ś'aka corresponds to our sâqa, i.e. ṭawwaqa" (p. 42). The verb sâqa (o) means "to drive (the cattle or the best), urge on, herd;" thus, "to stalk;" and ṭawwaqa "to surround, encircle." Furthermore, al-Iryânî does not prove that ś'k = sâqa. Then, in his commentary on ś'k/wqyd which he translates as "has stalked and hunted" on the basis of his ṭawwaqa although he had accepted the synonymity of ś'k and sâqa, he introduces an element foreign to both texts, viz. "their [= of the animals] ultimate fall into specially prepared traps." This new element comes from the word krwm which is mentioned, e.g., in Ir 41/3. But, that specification does not exist in Ir 42 and 49.

The formula 'lt/ś'k always begins an inscription. When it begins the second section of a text, it is preceded by the conjunction w and, as in Ir 59/2. Therefore, not only the conjunction must be restored in Ir 56 b/2 (cf. above, p. 52), but also the division of both Ir 52 and Ga 97 into two texts each is further justified by the beginning of text b as 'lt. Furthermore, because of the demonstrative value of 'lt, the end of Ir 50, 52b and Ga 97b must have included the tally of the hunt. Garbini notes that it is "illegible" in Ir 50 (p. 24) or has "disappeared" in Ir 52b (p. 24). In each case, the tally is represented by his dots at the end of his transliteration. This remark is even more valid in his treatment of Ir 64 because the whole end of the text was not registered. The tally of Ga 97b is not to be seen in the photo.

(b) Garbini actually starts his study of ś'k (p. 23) with "s₃wk (a root found also in other Semitic languages) with the meaning of "to surround with walls', 'to enclose'" (p. 23) without being aware that the variant śwk is already attested in RÉS 3943/5 and 14 (in the passive voice) with the meaning of "to encircle." This verb is already listed by K. Conti Rossini in his Chrestomathia arabica meridionalis epigraphica, Rome, 1931, p. 254B. Garbini regularly translates ś'k as "has captured" but simply as "captured" in Ir 54 (p. 31); he does not translate it in Ir 64 (p. 34) while he renders ś'k/wqyd as "has captured and helped to hunt" in Ir 53/1 (p. 32) or "has captured and helped to capture" in Ir 55 b/1 (p. 33). His translation of the verb as "to capture" is explained as follows:

> " s₃'k refers to a way of hunting or a particular stage of the hunt when the beaters form a circle at varying intervals to drive the animals towards an enclosure where they can be captured alive. For this reason, s₃'k has been translated as «to capture», a verb which does not imply the killing of the animals " (p. 23).

Cf. above, p. 54: al-Iryânî is more explicit as he injects the idea of traps. The verb ś'k = śwk does obviously not include any killing, but s(y)d does. Furthermore, the simple action of ś'k/śwk does not mean "to capture" because some of the chased animals will inevitably escape from the beaters, and the sole purpose of chasing beasts in a hunting party is to kill them. If not, what then does Garbini think the two mukarribs intended to do with more than 3700 wild animals? Garbini may rest assured that those hunts were not intended to provide the mukarribs with wild beasts to be kept in their zoos (provided they had any) or in their palaces or their servants' homes ... as pets! Then, Garbini tries to corroborate his description of a hunt by referring to a stone complex seen by P. Lippens in Saudi Arabia (cf. below, Appendix A, p. 60).

Garbini ends up his commentary on ś'k by stating that " It is evident that the true hunting, where the animals were actually slain, was an exclusive prerogative of the sovereign " (p. 23).

with reference to RycCRA. This reference is disregarded here because it does not give the page number corresponding to his statement. Garbini's comment is a manifest error because there must have been a great number of hunts by rulers as well as by chieftains and individuals. In each case, it must have been the prerogative of the sponsor of the hunt either to reserve to himself the killing or to invite some participants to do the killing with him. The present collection of texts offers three cases of the actual killing being done also by a follower of krb'l/wtr in Ir 52b, 53 and by the mukarrib's son in Ir 55b (cf. above, p. 41). Garbini's last re-

mark on Ir 53 proves that he was aware of the contradiction in his theory; yet he maintains it!

In his commentary on Ir 53 (p. 32) written by ʿmšfq who belonged to the generation after krbʾl/wtr's, Garbini explains his solution to his problem as a consequence of his position on the killing of wild animals. His opening statement is gratuitous, viz. ṣd and ṣyd "are different verbal forms." He makes this statement because, without acknowleging it, he is hiding behind W.W. Müller from whom he copied the idea, as reported by RycCRA, p. 276. Cf. the perfect parallel, viz. the two spellings, plain and defective, of the noun ṣd = ṣyd the hunt; e.g., in Ir 41/3 and 47/2, both by yṯʿʾmr/byn. Garbini continues his expose as follows:

"ṣd is the base form and has the meaning «to hunt», an operation reserved solely for sovereigns; ṣyd is a different form, semantically similar to the III form of Arabic, and suggests concomitance" (p. 32).

Here again, he erroneoulay restricts the use of ṣyd to the mukarribs. The idea of "concomitance" is already expressed by the conjunction w and. What is Garbini's justification for skipping the 2nd form, which in Ar implies intensity? What is Garbini's justification of his idea of "concomitance" for the 3rd form? The 3rd Ar form of a transitive verb indicates an effort to perform the basic act. In a last-ditch effort to prove his idea of "concomitance," he childishly states that

"it must be admitted that the two personages held a particular position in the court" (p. 32) and

"I therefore translate it as «to help to hunt», and see the text as meaning that the two personages who ś,ʾk wṣyd must have been, as it were, squires accompanying the mukarrib in the hunt" (p. 32).

This is an obvious "lapalissade" since a squire is by definition a male attendant, especially on a great personage; it is also a redundancy since l.3 explicitely says bʿm/krbʾl; but such an information does not affect the meaning of ṣyd in the least. But what is worse is that one of "the two personages" is yqhmlk, the son of krbʾl/wtr in Ir 55b. Of course, a prince of the blood has "a particular position in the court" (cf. already above, p. 48) and may accompany his father but most certainly not as his squire." The other man does not present himself as having any relation with the mukarrib, as do the woman ṣḥrt in Ir 43 or the two women of Ir 46b.

Garbini's solution of the second text, Ir 55b written by yqhmlk, krbʾl/wtr's son, is quite different (p. 33). The question of śʾk/wṣyd by yqhmlk is not even raised or mentioned here; not even a cross-reference to p. 32 where he discusses the social status of "the two personages." He interprets Ir 55a as written by yqhmlk also, although palaeography disproves the connection, "evidently as a token of loyalty;" cf. also al-Iryānī, who speaks of "devotion" (Ar p. 68). But, why would krbʾl/wtr's son

proclaim publicly his "loyalty" to yṯʿʾmr/byn while repeating twice in the very same text that he is the son of krbʾl/wtr? This does not make any sense. In addition, his interpretation contradicts his former statement reproduced above (p. 51) on "a de facto predominance of Karibʾil over the possibly milder Yathaʿʾamar." Were that predominance a fact, yqhmlk's public avowal of his allegiance to the losing party would have been completely foolish on two accounts. The situation of krbʾl/wtr's brother, mʿdkrb of Ir 56b, is practically the same as that of his nephew yqhmlk of Ir 55b. But, because of an untold, incomprehensible reason, yṯʿʾmr/byn's name of Ir 56a is no longer considered "evidently as a token of loyalty" by Garbini; it becomes "as a sort of title" (p. 30) of which, of course, no detail is given. How could the name of a ruler be or become "a sort of title"? After that, the fact that neither the name of yṯʿʾmr/byn's son, dmrkrb of Ir 60, nor yqhmlk is "a dynastic name" "suggests," according to Garbini, "that the sons produced by the mukarribs were automatically excluded from the succession to the throne" (p. 33)! But, since those names cannot be called "dynastic," this fact rather suggests that the new mukarrib took a dynastic name upon his selection, whether he was succeeding his father or not.

(c) The meaning of śʾk and ṣ(y)d.

The preceding remarks justify the opinion that they describe the two main parts of the hunt, viz. the beating up of the wild beasts toward their would-be killers and the killing itself. A nice Thamudic example of a beater may be seen in JaT 118p in JaMAR, XVI (1988), p. 232, bottom right. It should be noted that each beater does not seen to have acted in close concert with the others with the exception of driving the animals in one general direction and also that, although each beating up is claimed by a single person as his own, the latter must have been accompanied by some friends to make the action more efficient, as exemplified by three friends of ṣbṯm of Ir 54, viz. rʾšhmw of Ir 61/1, and both ʿmkrb and ʿmʾns of Ir 62 (cf. below). These two remarks indicate that the idea of "to encircle, surround" of śʾk/śwk cannot be appropriately applied to each beater and his companions; there must have been many gaps between the differents groups through which at least some beasts ran to safety. That two persons took part in both the beating and the killing is easily understood: they left the other beaters when the process of driving the animals to the would-be killers had progressed far enough for them to safely leave their place, rejoin the hunter(s) and do the killing with him/them.

(3) The five sigla Ir 44, 55, 56, 59 and 60.

The situation of the authors of Ir 55b and 56b is studied above (pp. 56-

57). These two texts do not actually contain any indication of some crossing over to the other *mukarrib* by the two relatives of krbʾl/wtr. But crossing-over did happen as seen in Ir 44, 59 and 60.

In the first text, it is gwrkrb, a nephew of krbʾl/wtr, who went to beat up for ʾt-ʿʾmr/byn, and it was his only participation to any hunt. Maybe -who knows?- it was the young man's first opportunity to take part in the excitement after growing up enough to be an efficient, safe beater.

In Ir 59, it is ʾbkrb who went to beat up for his brother yt-ʿʾmr/byn and then for krbʾl/wtr. Each of the two mukarribs is introduced by the formula ʾlt/bʿm and is followed by the tally of the beasts.

Finally in Ir 60, dmrkrb was a beater for his father yt-ʿʾmr/byn and krbʾl/wtr, and each of the two names is introduced by the compound preposition bʿm only, not by ʾlt/ʿm as in Ir 59.

Therefore, the presence of a single tally after the mention of the two *mukarribs* combined with the absence of a second ʾlt, as in Ir 59, suggests that the hunting party to which dmrkrb participated actually was a hunt sponsored by the two mukarribs (cf. already above, pp. 41-42).

5 - The location of ʾrydy and drm.

In his commentary on Ir 51 (pp. 23-24), Garbini develops his theory on the location of ʾrydy and drm.

a - ʾrydy.

"The hunt ... took place there" = "in the gorge of al-ʿAql" (p. 23); the name ʾrydy (cf. Ar rayd "a ledge of a mountain") "is perfectly suited to the gorge of al-ʿAql" and "finally "the fact that the inscriptions were written there makes it very probable that ʾrydy was the ancient name of al-ʿAql" (p. 24). Here again (cf. already above, p. 33), Garbini does not utilize the whole epigraphic material he himself publishes; in fact, he ignores the very important information of Ir 48/3 (cf. above, pp. 25-26) according to which the author dug a well at ʾArayday. Therefore, ʾrydy cannot be sought in the rocky J. as-Saḥl in general and at al-ʿAqûl in particular; it has to be sought in the lowlands where a well can be dug.

b - drm.

Thanks to his improbable etymology of drm from the root "dny" (there is no nominal derivation which includes the apocope of the last weak radical), Garbini suggests that drm might be "the enclosure where the captured animals were finally gathered after being channelled along the corridors (in this case the gorge of al-ʿAql)" (p. 24). Therefore, for Garbini, drm might be the reserve or corral or zoo "where the captured animals were finally gathered"!!! But the texts deal with hunting parties, not

with conservationists' expeditions aimed at collecting specimens for reserves. Furthermore, Garbini's presentation of drm as the second stage of the hunt begun in ʾrydy (= al-ʿAqûl) is contradicted by at least 5 texts (cf. above, p. 41) each of which records a hunt at either drm or ʾrydy, and the killing of animals also occurred at ʾrydy according to Ir 42.

(3) de Maigret's endorsement of Garbini's thesis.

The preceding pages point out in detail - and more follows - the erraticism of Garbini's theories on the so-called ritual hunts practiced by the mukarribs. If de Maigret remains silent on several aspects of Garbini's theory, - therefore, the reader has no way of knowing for sure whether or not he agrees with them although the affirmative answer is more than likely - , he at least endorses Garbini's basic understanding of the hunting parties and applies it to the archaeological complex. He speaks of

" the concrete evidence of ancient worship-related practices. The inscriptions confirm this and point to the importance and type of ritual which characterized this kind of activity. It was in this very gorge of Ğabal as-Saḥl that the *mukarrib* carried out the "holy hunt". The villa on the lower site must have been the residence of the sovereign during the hunting seasons and the path carved out on the right side of the gorge was the way to the place set aside for the cult and, perhaps, for the practice of the ritual, in the upper site" (pp. 17-18).

Since there is no reason for stopping on the road of such beautiful speculations and in compliance with the adage "ab uno disce omnes," de Maigret does not hesitate to suggest that

"The question should be posed as to whether, in this archaic period, the various isolated mountains on the edge of the Yemenite desert were perhaps each seen as actual seats of distinct religious rites" (p. 18).

Finally, he summarizes his whole conception 24 lines below saying that

" Within the settlement, Šiʿb al-ʿAql was devoted to cultual and ritual activities" (p. 18).

The whole scenario is quite bewildering. It is in the nature of wild beasts to steer clear of any inhabited area. But, astonishlingly enough, those animals apparently made several exceptions to their natural way of life: they went "in this very gorge" where the "mukarrib" or "sovereign" had his hunting "residence" for the manifest reason (what else could it be?) of accommodating the rulers who needed them for their so-called ritual practices. Another possibility of explaining the "holy hunt" "in this very gorge," which is much more in the line of the inscriptions, is even much more irrealistically impossible and terrifying than the first because the lives of all the inhabitants of the complex would voluntarily be put in extreme dan

ger, viz. the beaters led the wild beasts into that inhabited gorge where they we-
re trapped and slaughtered. Only the thought of 1000 wild animals (cf., e.g., Ir
54/3) trapped and killed in that narrow inhabited gorge is already frightening en-
ough, but the actual reality of it would be a horrifying nightmare for the inhabi-
tants. In addition, de Maigret's use of the two words "mukarrib" and "sovereign" in
the singular proves that he failed to understand Garbini's translation, provided of
course that he read it, - but Garbini himself did not realize the importance of the
text -, that one hunt sponsored by TWO mukarribs is attested in Ir 60. So, if the-
re was a "residence" for one mukarrib in "the villa on the lower side," the reader
would have liked to know where de Maigret puts the lodging of the second mukarrib
and his retinue since, according to him, "the upper site" was "for the practice of
the ritual." Moreover, I have seen many "isolated mountains on the edge of the Ye-
meni desert" without any pre-Islamic remains. Furthermore, not a single religious
inscription and/or artifact was found there, and a staunch fanaticism only can see
a "holy hunt" in short narratives of ordinary hunting parties (cf. already above, pp.
30-31). de Maigret may find consolation in the fact that he is after all following
into the footsteps of the laudemus invicem club members who still sheepishly hold on
the ideas of the antiquated fancy of A.F.L. Beeston's 1948 paper (cf. above, p. 39);
an overall presentation of the ideas of those club members may be seen in J. Ryckmans'
sibylline jumble in W. Daum ed., Yemen [English version], Innsbruck, 1987, pp. 107-
110, where the reader finds among many erratic statements the fallacy that "a ritual
hunt performed by the ancient mukarribs" (p. 107B) "is well attested" (p. 110A). Fi-
nally, I wish to refer de Maigret to JaMAR, XII (1980), pp. 69-71, which he ignores,
for my answer to C. Robin's article listed in his footnote 5 of p. 17.

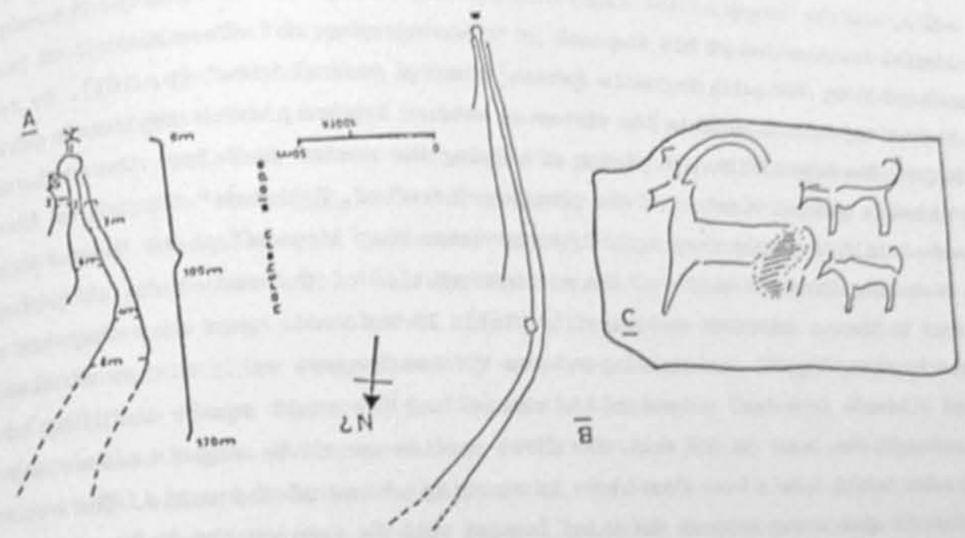
A p p e n d i c e s

A - P. Lippens' stone complex (cf. above, p. 55).

The stone complex seen at the northern extremity of Laḥyâ al-Jimâl, "les mâ-
choires des chameaux" (about 85 km southeast of Bishah, Saudi Arabia), during the 1951-
1952 Philby expedition, is described as "ayant servi à la capture d'animaux aux temps
préhistoriques (?);" cf. P. Lippens, Expédition en Arabie Centrale, Paris, 1956, p.
67. The other members of the expedition did not see the complex which was nonetheless
published by J. Ryckmans in PSAS, 6 (1976), pp. 161-165 (with four sketchy figures on
p. 162) as "an ancient stone structure for the capture of ibex" (p. 161). Follow on
the next page the first three figures of Ryckmans' paper.

1 - The walls (cf. fig. A and B).

The very first objection comes from Lippens' testimony according to which
there are "cinq murs en moellons, variant de 50 à 200 mètres de longueur;" he also



speaks of "murets" (cf. l.c.). But there are only four in both fig. A and B, and
Ryckmans' description of the complex (pp. 161 and 163) mentions four only. Where
is the fifth one?

Fig. A - The caption states that it is "from Ph. Lippens' original sketch." Ryck-
mans comments on it (p. 161) that it was "not drawn to scale" and that it
was reproduced with "some annotations being left out for clarity." First of all,
Ryckmans does not say how it was reproduced; nor does he spell out the changes in-
flicted upon the original. E.g., the tall vertical brace, the expression "northern
slope" and the number of the measurements were added (the letter "m" of both fig.
were typed with the same machine). The end of the left channel is blocked in fig.
A by a square that is twice too large because Lippens himself specifies that it is
"une cage en pierres plates d'un mètre cube" (cf. l.c.), and the distance between
the two parallel walls is 2 m. In Ryckmans' sketch of fig. B, that opening is clo-
sed by the extreme left wall which bends rightwardly. What else did Ryckmans alter
in Lippens' facsimile? Therefore, fig. A cannot be considered as representing Lip-
pens' sketch. In addition, the scale of fig. A is completely off: the distance bet-
ween "0m" and "100m" is 1.85 cm, but that between "100m" and "170m" is 2.35 cm!

Fig. B - Although Ryckmans claims that his own fig. B is Lippens'(?) fig. A "re-
drawn to scale" (cf. caption of fig. B, p. 162), his scale also is off:
the shortest wall measures 26 m only (instead of "50"); if the exit of the longest
channel is "2m," then its entry is 10 m wide (instead of "8") where the stippling

begins, and the length of the longest wall is 213 m (instead of "200"). The only material Ryckmans had at his disposal is "a photograph - not clear enough to be reproduced here, but quite legible - [which] gives a general view" (p. 161). No reason is given to substantiate the statement between hyphens, which implies a contradiction. The main affirmation aiming at allying the reader is false: the photo does not show "a general view" of the complex; therefore, Ryckmans' tracing of the lower half is simply imaginary. Ryckmans notes that Lippens' photo "seems to have been taken from the middle of the straight section of the structure, or perhaps nearer to the circular enclosure" (p. 163). If the words mean what they are supposed to mean, Lippens was standing between the two longest walls either at almost equal distance from the exit of the channel and the small square building on the second wall or closer to the exit. The first section cannot be right because any photographer would take a view from above by standing on one of the walls. The second section is also wrong because the third longest wall is seen on the left of the photographer; and its length suggests that the photographer was standing toward the beginning of the shortest wall. Finally, Ryckmans guesses that "the direction of the sun in the picture ... makes me assume that the camera was pointed more or less towards the north-west to north, in contrast to the very general orientation in the original sketch" (p. 163). Since Ryckmans' fig. D does not show any shadow, his orientation cannot be checked and is therefore dismissed. But the reader wonders why Ryckmans did not ask Lippens all those questions on the photo instead of indulging in speculations? And Ryckmans' second statement is erroneous since there is no orientation in fig. A. In addition, the caption also specifies that "an additional feature, visible on the photograph, is noted" = "a square ruin, not recorded in the original sketch, appears near the middle of the western corridor" (p. 161). Sketchy fig. B puts it on the left wall of the right channel, but in Ryckmans' sketch of the photo in sketchy fig. D the slightly convex line of the hilltop making the horizon prevents the onlooker from seeing anything of the lower half of that feature; therefore, whether it is a wall making a square or only a bent of the wall itself remains to be seen. Ryckmans continues saying that "it is built of roughly dressed stones, and could therefore be independent of the original structure" (p. 163). But what can be seen on sketchy fig. D is only "roughly dressed stones." Therefore, the true purpose of Ryckmans' remark is to eliminate a feature which would make difficulty for his overall thesis. It is most probably having the preceding feature in mind that Ryckmans writes that "there are discrepancies between the data of the sketch and of the description, on one hand, and what can be made out from the photograph" (p. 161). Of course, Ryckmans does not give any reason or reasons to justify his deduction; nor does he itemize the sections which, according to his

cryptic revelation, were altered or added. Summarily, the reader does not know what Ryckmans accepts of the original, and how much he distorts Lippens' testimony. The basic fact remains that Lippens drew what he saw, and that Ryckmans saw nothing.

It is remarkable that Ryckmans remains silent on the width of the walls, but he claims that their height is from 50 to 100 cm (p. 161). The reader wonders where Ryckmans got that information since the sketchy fig. D is useless and Lippens speaks of "murets" (cf. l.c.). Most importantly, he concedes this: "the ruins do not suggest that the walls were by themselves high enough to form a serious obstacle to the agility of an ibex" (p. 164). Ryckmans apparently did not understand what he wrote because it simply demolishes his other irrealistic statement according to which "the various corridors were apparently conceived to break up the party of animals, and facilitate their capture" (p. 164) as if, upon seeing the walls, the wild beasts would quietly slacken their speed to a gentle trot and finally to a walking pace. Already at full speed because of the pursuers, the beasts would have gotten into a stampede, if they were not already in, and low walls forming converging channels would have been as useless as lines of fagots in the wild boar hunting. The beasts would have kept on running straight ahead and jumped over the walls to safety; and no sane beater or hunter would relish to be in the path of those charging animals. In addition, "and facilitate their capture" does not make more sense than the rest. The width of the channels is said to be from 8 to 2 m (according to fig. A) for a length of 50 to 200 m (cf. Lippens, l.c.). Thus, a mere score of frenzied beasts would suffice to clog the entrance of the longest (the right) channel. If such is the case, it would be most ridiculous to build up two walls of 200 m long each for so few beasts. But since those walls are there, Ryckmans actually imagines that the frenzied animals would quietly trot the whole length of the channel instead of jumping across the low walls! Furthermore, where would Ryckmans' "capture" take place? Incidentally, note that "capture" does not at all imply "killing." It is at this point that Ryckmans tangles up the whole situation. The two lateral channels have each an unhindered exit; only the central channel ends up in "a circular enclosure (diameter: 3.5 m)" (p. 161). But, Lippens speaks only of "3 mètres de diamètre" (cf. l.c.)! Convinced that the walls were not high enough to stop the ibex, Ryckmans is forced to invent that "most probably hunting nets" (p. 164) were used in two places, viz. "at the narrow end of the western corridor" and "on the wall of the circular enclosure" (p. 164). Beside forgetting the exit of the left channel which remained open, Ryckmans does not even see the illogicality of the situation: if the wall of the circular cubicle was not high enough to stop the ibex, why was it built in the first place? and what was it for? It was simply superfluous since there is no such feature at the exit of the two other passages.

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Furthermore, the use of nets renders Ryckmans' thesis even more ridiculous. It would take no more than half a score of ibex to fill the two nets, and then what? The animals had to be killed and removed where? In the meantime what does Ryckmans think the other beasts were going to do since the passage was clogged? Sit down, hold their breath, and maybe take a nap until the nets were ready again to receive some more visitors? How ridiculous!

Another point to be examined here is the most inconsiderate statement seen so far in Ryckmans' paper. He deals with "the various corridors" and says this: "insofar as they all actually belonged to the original structure" (p. 164). But such an affirmation is most irresponsible because those three channels a r e "the original structure" for the simple reason that without them or beside them there is nothing, and the three of them together form a well-conceived sorting device.

The last query is the nature of the feature just above the circular enclosure with which it is connected by two short, parallel bars and which becomes some kind of asterisk in Ryckmans' fig. B. Ryckmans does not even mention it.

2 - The isolated petroglyth = fig. C.

Ryckmans simplistically takes the design of an ibex pursued by two hunting dogs on a slab found "on one of the walls of the central corridor" (p. 163) as "a striking indication as to the original destination of the structure" (p. 163). He does not even know to which wall the stone belonged to, and how can he speak of "the central corridor:" none of the four sketchy figures indicates the former location of that stone. Lippens says only that the flat stone "était posée à même un des murs"(cf. l.c.), and the phraseology clearly indicates that, according to the author, the stone appeared not to belong to the wall.

In addition, what is really "striking" is Ryckmans' impertinent pharisaism in lamenting as follows: " as long as the original stone found by Lippens is not available for inspection or for a photograph" (p. 164). It takes a person literally bursting with impudent pride, such as Jacques Ryckmans or A.F.L. Beeston (cf. JaMAR, XIV [1985], pp. 80-82 and 90-93) to publish such an affirmation. Even the first reading of Ryckmans' paper leaves an impression of uneasiness that comes from the fact that Jacques Ryckmans knows nothing about the stone complex except the twelve lines of Lippens' narrative (cf. already above, p. 62, the questions relating to Lippens' photo). But how could the latter have made that discovery without the knowledge of the other members of the expedition? He went back to the camp "en portant, sous le bras, une dalle" which could not be hidden. Yet, Jacques Ryckmans did not make any inquiry; he did not go and take a look at the place; he did not take any photo; he did not make any sketch of the petroglyth; he did not do a thing. But this is not the

the end of the story. That very stone remained in the custody of the expedition for more than two solid months. Indeed, the stone was seen about December 5 or 6, 1951; the expedition left Riyadh on February 15, 1952; and Philby still had the stone with him at that time. Yet, during those two long months, again Jacques Ryckmans did not do a thing; viz. no photo, no facsimile, no information, no measurement, nothing. Yet, Jacques Ryckmans does not shrink from stating "For some reason no photograph ... was made by Lippens" (p. 163)!!! Is it possible to be more selfishly blinded and bias? And Jacques Ryckmans laments about a vacuum of information of which he is as responsible as his uncle. That attitude of the two Ryckmans that lasted for more than two solid months is unequivocal: they could not have proven more clearly that they could not have cared less about that complex and that stone. Yet Jacques Ryckmans' statement also implies that it is up to Saudi Arabia to take the initiative of making the stone "available for inspection or for a photograph" to him in Belgium who had at his disposal about seventy days to do that simple job, but could not care less, or to E. Anati in Italy. What a demented egotism! Furthermore, what is also "striking" is Ryckmans' illogicality. On the one hand, Lippens' facsimile of the petroglyth (fig. C) is said to be "inadequate" "to identify precisely the style of the hunting scene" (p. 164). What Ryckmans had in mind is the very diversified array of stylistic labels devised by E. Anati (cf. JaMAR, I [1971], pp. 71-81); and what his statement says is that Ryckmans was unable to find in Anati's store of stylistic tabs one that would fit the stone petroglyth. Why did he not ask Anati himself who surely must have many more still unused stylistic stickers in his imagination? On the other hand, the same facsimile was not "inadequate" for Ryckmans' simplistic mind to connect the stone with the complex. This is naive simplemindedness because he should have proven that the stone actually belongs to the complex (cf. above, Lippens' statement on p. 64), viz. that it was not, and could not have been, added later or that the petroglyth could not have been engraved at a later period.

Moreover, Ryckmans is so blinded by his theory that he did not even notice the "striking" difference between the two hunting methods. The scene on the stone may belong to the flatland hunting where dogs may be usefully engaged provided that the ibex is not too far away. If the ibex is far away or in a rocky mountain, which is its natural habitat, the dogs are practically useless because they are immediately outrun and outclassed: they cannot manage the cliffs and escarpments as ibex do. The effectiveness of dogs is to considerably widen the beaters' field of action. Once the ibex are cornered in a gorge or ravine, the dogs rush in

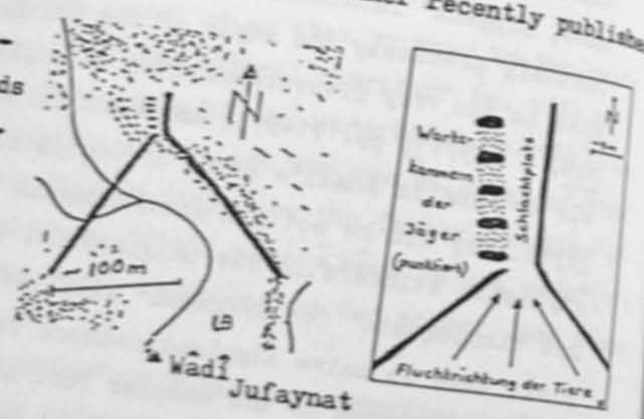
for the final attack. Such a case, however, which could also be the motive of the stone design, cannot be related to a humped hillock where the beasts cannot be either stopped or trapped. Of course, the design could also depict a strayed animal surprised by dogs.

3 - The purpose of the stone complex.

It seems quite obvious, at least to me, that two long, narrow channels like the two main passages of the stone complex had to be devised exclusively for destinated animals; wild beasts would immediately obstruct the entrance of each channel because of their speed and their frenzy. The stone complex could have been used for sorting out either the best individuals of a herd of goats or sheep for husbandry and/or hair/wool purposes, or the weaklings, etc. Then, the narrowness of the last 42/43 m of the three channels, viz. 2 m (according to fig. A), becomes understandable as allowing the onlookers standing on the walls to start the selection process of the animals even before the three channels were filled.

B - U. Brunner's "pre-sabaean hunting snare."

Without any reference to Lippens' stone complex, Brunner recently published in Jemen-Report, *20, Heft 1, 1989, pp. 23-25, what he calls "vorsabäische Jagdfallen" found in the undulated lava fields southwest of Märib. Here are his two facsimiles.*



Even at first sight, it appears , exactly as in Ryckmans' paper (cf. above, pp. 61-62), that the two scales are different, viz. the length of the neck would be about 55 m in fig. 1, but only about 18 in fig. 2; and the angle made by the two walls is 75° in fig. 1, but 95° in fig. 2; the length of those two walls is not fully drawn in fig. 2. Here again, as in Ryckmans' paper, no accurate detail is given by Brunner, although he claims to have inspected the place (p. 23A), on the most important features of the system, viz. the height and width of the walls which, at least in photo 1 (p. 23B-C), look very low; if the sketch of fig. 2 has any value, the width of the wall is about 50 cm (cf. also below). Here again, as in Ryckmans' paper, it is not with such low walls that hunters will stop a herd of stampeding beasts; they could jump over it in the twinkling of one eye. Brunner writes in his summary (p. 23) that "die kleinen Mäuerschen dienten als wirksame Fluchtlinien für das Wild (v.a. Gazellen)" (my underlining). The completely gratuitous specification of "Gazellen" comes from Burckhardt's description of a hunting sche-

me in the Syrian desert (cf. below). The main question is not to direct the wild animals, but to restrain them and finally to break their run so that they might eventually present themselves by twos or threes at the neck entrance, but the thrust of stampeding wild animals cannot be broken by mini- or micro-walls. Brunner's thesis is also irrealistic because of another obvious reason of which he does not even speak, viz. the exit channel is so narrow, about 17 x 3.5/5.5 m (if fig. 2 has any value), that it would be blocked by even less than a dozen of dying or dead animals. Brunner does not say who was going to remove them from the passage, and what kind of charm or soporific would be used to keep the other frenzied beasts quiet until the channel was reopened.

Then, Brunner springs on apparently in all confidence in the SA epigraphical field of which he knows nothing, and he picks up here and there from an obsessive of the so-called sacred/ritual hunt bits of information, unable as he is to check what he selected for accuracy.

He firstly refers to R.B. Serjeant, South..., "ann. 130" as an example of Reiseberichte von verwendeten Wüstendrachen" (p. 243). The purpose of Serjeant's note 130 (pp.97-98), the reference of which Brunner copied from Ryckmans (cf. below), is to explain the author's novel interpretation of bdltn of RÉS 4782/2, a text which does not speak of any kind of hunting snare. For RÉS 4782, cf. JaMAR, XIV (1985), pp. 82-84; F. Bron's paper in SEL, 6 (1989), pp. 123-125, which ignores the preceding study, was already answered in JaMAR, XVI (1988), pp. 81-82, which he also ignores. It is inside a long digression on wa'il, etc, that Serjeant sandwiches a testimony from eastern Syria on hunting gazelles which "were led into traps by stone walls, and killed" (p. 98). Such a sketchy information does not contain anything new, and the lack of detail almost makes it useless since we already have Burckhardt's detailed narrative, but we do not even know whether or not the new sheme is similar or different from Burckhardt's.

After that, Brunner refers to Ryckmans' ridiculous assumption (cf. RycCRA, p. 261) according to which there is a relation between the so-called ritual hunt and the fertility of the forthcoming rainy season. But, according to his usual trick, Ryckmans does not even attempt to prove his concoction or to inform his reader that the assumed connection is not attested in a single text.

In his final remark on the subject Ryckmans claims that "un rapport analogue" does exist in a few texts "où la décharge honorable de la fonction" of rēw = priest "semble liée à l'octroi par le dieu d'une irrigation convenable, exprimé par la phrase: 'et ʿAttar a abreuvé Saba au printemps et en automne'" (p. 261). Note

first that Ryckmans is not even capable of respecting the order of the two words ḫrf/wdṯ' which refer to the crops harvested during the two seasons, and not at all to the seasons themselves. Secondly, the adjective "analogue" implies similarity. Ryckmans' statement is untrue. By distorting the original text, Ryckmans prepares the terrain before introducing his new error. It really is as if Ryckmans could not touch a single text without distorting it. His statement is followed by a reference where he formally states this (cf. BiOr, 24 [1967], p. 143B):

> " D'une autre formule, wsqý 'ṯtr sb' ḫrf wdṯ', "et 'Aṯtar a abreuvé Saba en automne et au printemps", il découle que l'éponyme avait des fonctions importantes en rapport avec l'irrigation."

The reader should not be surprised if he does not understand exactly the meaning of Ryckmans' statement (vague phrasaeology is normal in Ryckmans' publications) beside an indefinite, quite nebulous relation of dependence. In fact, the relation that exists is one of contemporaneity. Here follows a typical example, viz. Ja 2848 ar/5-8 (cf. my Carnegie ..., p. 55):

"5 ← ʿmkrb/bn/wddɔl/bn/ṣbḥrm/ḏhll/
6 ← ywm/rš̌w/ʿṯtr/wfdyhw/bn/kl/ɔby
7 ← thw/wsqy/ʿṯtr/sbɔ/wgw(m)
8 ← ḫrf/wdṯɔ

5 ʿAmmkarib, son of Waddâdɔil, son of Ṣabaḥrâm, he of (the tribe) Ḥalil,
6 when he was the priest of ʿAṯtar, and has freed himself from all his
7 houses, and ʿAṯtar has irrigated Sabaɔ and the community,
8 the autumn- and spring-crop."

Brunner then refers to "Ryckmans (p. 270)" = RyCRA, who lists L.W.B. Rees in Antiquity, 3 "1929: 400-401," who in turn quotes J.L. Burckhardt, Notes on the Bedouins and the Wahábys, London, "1831: 200 s." Ryckmans (pp. 270 and 304, with "I" = 1st vol.) blindly copied Rees' footnote of p. 400 and its four errors, viz. (1) in the title, read "Wahábys" instead of "Wahabys;" (2) the book was published in one volume (thus, the mention of "I" is erroneous); (3) the book was published in 1830 (not "1831," as also in National Union Catalog, Totowa, NJ, 1978, vol. 17, p. 342C). Points 1-3 are plainly visible on the book itself and correctly mentioned in British Museum General Catalogue of Printed Books, London, 1965, vol. 29, col. 1087); and (4) the quotation comes from p. 125 (not "200, 201" = "200 s."). In addition, the word "masiade" comes from Burckhardt as his transliteration of the Ar word مصيادة (p. 125, line 4), not from Ryckmans, as claimed by Brunner. But, Ryckmans adds a gross error, viz. "maṣyada, pl. de maṣyad" (p. 270). In fact, maṣyadat =

maṣyad, of which the plural is maṣâyid. Note that Burckhardt's text is reproduced by S.W. Helms, Jawa, London, 1981, p. 41, with the omission of "purposely," and of two commas, the addition of an article "the," and the wrong date "1831." Furthermore, in order to eliminate a major objection from Burckhardt's description, Ryckmans' summary of the latter discards the important fact that "the enclosed space is situated near some rivulet or spring to which in summer the gazelles resort" (cf. Burckhardt, l.c.). This characteristic feature justifies not only the building of a permanent high wall closing three sides of a large area, but also the digging of large ditches; without that "rivulet or spring" there would not have been any "high wall" or "deep ditch." But it is the same distorting mind which ends up his summary of the aforementioned hunting scheme with this: "cf. J. RYCKMANS 1976b: sur un dispositif apparenté en Arabie Centrale, destiné à la capture des bouquetins" (p. 271). This reference is precisely where he publishes Lippens' stone complex (cf. above, pp. 60-66). The use of the expression "dispositif apparenté" is a deliberate whopper because the essential part of the hunting scheme is described by Burckhardt (p. 125) as follows: "in different parts of this wall gaps are purposely left, and near each gap a deep ditch is made on the outside." Even at first sight the preceding system appears radically different from Lippens' and Brunner's complexes which both have very low walls and no openings in different parts of the walls. As if to confirm that the aforementioned distortion of the facts is normal in his papers and especially in this one which he himself calls "une étude critique" (!!; p. 261), Ryckmans releases immediately after his preceding judgment that which, "à notre avis" (p. 261), "paraît être" (p. 262) the true interpretation of Gen 27/1-40 (pp. 261-262). It is so extravagant and ridiculous that all that Ryckmans apparently is capable of is to distort any text or testimony in order to fit his own imaginary concoction instead of understanding it as it is.

Furthermore, Brunner's remark to the effect that the question should be clarified as to whether pits are detectable in the area of the neck (p. 24C) is most inappropriate and superfluous because (1) a deep ditch or trench can easily be spotted on a photo; (2) there cannot be any ditches since there are no gaps in the walls ... unless, of course, they be located immediately behind (= west) the four roomettes or cubicles which Brunner calls the hunters' lookouts, and which measure each, if fig.2 has any value, about 3.5 x 2 m, as if each hunter was supposed to inform any passing-by wild beast "This way, please!;" (3) as Ryckmans, he apparently published a complex with insufficient information, but since he claims to have inspected the place (cf. above), the reader does not understand that he should ask such

an important question now and/or that he did not obtain from the German mission the necessary information before writing his paper; (4) his question has no effect whatsoever on the presentation of his thesis; and (5) last but certainly not least, it is impossible to understand how he did not see that the same complex in ABADY, I (1982), pl. 24 a and b, could not possibly include a single "deep ditch" since the low walls rest upon sheer rock. This remark is also valid for the four photos published by Brunner because the lava fields are in the mountains west of Mârib, and no "deep ditch" can obviously be dug there.

It is very strange that Brunner's "Literatur" does not include J. Schmidt's presentation of the "Y-Formen" in ABADY, IV (1987) 1988, pp. 166-174. There is enough blank space on p. 248-B of Brunner's paper for that information which is by no means superfluous.

Brunner's penult remark (p. 24C) is to the effect that were "deep ditches" detectable in the scheme shown in his photos, then RÉS 4177 (cf. above, p. 38) could find a connection. But Brunner's interpretation of his so-called "hunting snare" is materially impossible and, therefore, RÉS 4177 connot be connected with something that does not exist.

Brunner ends up his paper (p. 24C) in an irresponsible manner. Although knowing nothing in epigraphy and, therefore, incapable of discussing even a single word, he nonetheless rejects W.W. Müller's interpretation of krwm of RÉS 4177/4 ... and his sole justification is that the translation of his choice ... suits his thesis better!!!

Summarily, Brunner's paper is very regrettable because its superficiality cannot but unfortunately cast a long shadow on his previous contribution which fills up ABADY, II (1983), almost completely.

B - The remaining texts related to the two mukarribs.

1 - krb'l/wtr's hunts.

a - hlkm's family.

For the introductory question of the hlkm's family, cf. above, pp. 19-21. The four texts Ir 44, 50, 57 and 58 were engraved by the members of the hlkm's family, viz. the father hlkm (Ir 57), who was a brother of krb'l/wtr, and his three sons: 'db'ly (Ir 58), smh'mr (Ir 50) and ẖwrkrb (Ir 44; cf. above, p. 51). Three of them were beaters in the hunts while 'db'ly did not take part in any hunt since the text does not include the verb s'k.

Ir 50: Garbini, p. 24; al-Iryânî, Ar p. 64; general area in pl. 11b, left, and pl. 12; pl. 39b, upper right; for some remarks, cf. above, p. 6; for the question of Ma, cf. above, pp. 32-33; cf summary above, p. 41; for the remarks to the effect that the text is incomplete, cf. above, p. 54; for hlkm, cf. above, pp. 18-19 and 24.

's't/s'k/	←	1	These has beaten up
smh'mr/bn	→	2	Sumhu'amar, son of
hlkm/b'm/	←	3	Halkum, with
krb'l[/]dn	→	4	Karib'il at Dan-
m/w'rydy	←	5	am and 'Arayday.

al-Iryânî adds a word divider at the end of l.2. Garbini states that "the character h is of the North-Arabic type;" he makes the same remark on Ir 59 (cf. above, pp. 52-53). This type of h, Y , is well-known in SA epigraphy; cf., e.g., CIH 367/2. Both Garbini and al-Iryânî record "b'l/d" in l. 4. But, either l or / is missing; there is no room for the two of them; compare the distance between ' and d (3.2 mm) with that between s and b in l. 3 (5 mm). I assume that it is the word divider that was forgotten, as in Ir 53/2, 55 b/3 and Ga 95.

Ir 57: Garbini, p. 29; al-Iryânî, Ar p. 69; below Ga 95 and Ir 62 and to the right of Ir 58; pl. 14, bottom center; pl. 38, bottom of the second panel from the left; and pl. 41, bottom right; for some remarks, cf. above, p. 6; for hlkm, cf. above, pp. 18-19 and 24; cf. summary above, p. 41.

1 /'lt/s'k/hlkm/bn/dmr'ly/'m/krb'l/

2 wtr/dnm/w'rydy/$$/

1 These has beaten up Halkum, son of Damar'alay, with Karib'il

2 Watar at Danam and 'Arayday: 200.

Ir 58: Garbini, p. 29; al-Iryânî, Ar p. 70; to the right of Ir 57 and below the second half of Ir 62; pl. 14, bottom left of the central panel; pl. 38, bottom of the extreme section; pl. 41, below and to the right of Ir 46b, below the end of Ir 62 and to the left of Ir 57; for some remarks, cf. above, pp. 6 and 21-22; for the reading of 'rydy, cf. above, p. 13; cf. summary above, p. 41.

'db'ly/hlkm/yl'y[n]	1	'Adab'alay, [son of] Halkum, the Yal'ayite, [was]
'm/krb'l/wtr	2	with Karib'il Watar
'rydy/	3	at 'Arayday.

L.1: Garbini's reading of "'mr'ly" is unjustified. The gentilice yl'y must be followed by the article. The upper left vertical stroke of the letter n seems to be visible.

Another example of Garbini's incompetent reasoning is this:

"A town called *yl'y* is recorded by Karib'il Watar among his conquests (RES 3945, ll. 10 and 12). Since the ancient name of the town was Hîry (see below, Y 3), Yalî could be the name of the region around it."

Y 3 = Ir 48. The expression kl/bḳ'/yl'y of RÉS 3945/10 indicates the whole territory of the city yl'y and, as usual, that territory does not have a special name. Furthermore, the two fragments of Ir 48 were not found in situ; they could

have been transported from any other place; they are therefore useless to determine the value of *yl²y* of the present text (cf. already above, p. 26). – L.3: Garbini restores "[dm/w]" at the beginning of the line. This restoration is disproved by Ir 54, another text engraved by a servant of the same krb²l/wtr, which testifies that the *mukarrib* made a hunt at *²rydy* alone. Furthermore, the rock to the right of *²rydy* looks very smooth; were letters there, they should still be legible. Garbini's three dots following *²rydy/* suggest that the text is incomplete. But the photo of pl. 41 shows that the spot to the left of *²rydy* is very rough. In addition, nothing is missing in the text since no tally is expected because *²db⁶ly* did not *g²k*.

b – Note on krb²l/wtr's family.

The preceding study of the krb²l/wtr's relatives who took part in the hunting parties makes it possible to throw some light on the age of some of those persons. The *mukarrib*'s son *yqhmlk* (Ir 55b) participated in a hunt apparently as a full-fledged beater with his father (cf. above, p. 51). But his own son *drḥn* (cf. above, p. 18) was still quite young when he went under good protection to one of the places of the hunt as the text commemorating his visit is engraved at al-Jafnat. The *mukarrib*'s brother *hlkm* (Ir 57) did the same as *yqhmlk* with the *mukarrib* (cf. above, p. 71), but he seemed to have been a much younger brother because one of his sons, *²db⁶ly* (Ir 58), did not participate in any hunt probably because he was too young although he belonged to the same generation as his cousin *yqhmlk*, but he nonetheless went to *²rydy*. Furthermore, a brother of *²db⁶ly*, *ṯwrkrb* (Ir 44; cf. above, pp. 51-52), took part in a hunt sponsored by the other *mukarrib*, *yt⁶²mr/byn*. If he went beating for this *mukarrib*, it may well have been because it was his first opportunity to do so (cf. above, p. 58). The two visits by *²db⁶ly* and *drḥn* appear to indicate that there was somewhere in the area of the hunting grounds a country lodge to which krb²l/wtr retired at leisure and which was his pied-à-terre during his hunting parties.

c – *ṣbḥm*'s group.

The *ṣbḥm*'s group was already mentioned above (p. 57) as exemplifying the fact that each beater was not alone in the beating up but must have been accompanied by either some relatives and/or friends. His is the first group of persons not related to the *mukarrib*'s family, who took part in the latter's hunts.

The identity card of *ṣbḥm/fqḍn* of Ir 54/1 is complemented by Ir 61/5, *dmdbḥm* (cf. already Garbini, pp. 22 and 31), which is the common link *ṣbḥm* had with *r²shmw* of Ir 61/1. Although containing two distinct sections, ll. 1-3 and 4-5, Ir 61 was engraved by the same person and this fact, in turn, testifies to the good relation existing between the two men and also suggests that *r²shmw* took part with his friend *ṣbḥm* in his hunt with krb²l/wtr. Furthermore, the community of professional status, *fqḍ*, also suggests that *ṣbḥm* took along with him to the hunting party both *⁶mkrb* and *⁶m²ns*.

The most likely link *ṣbḥm* had with the *mukarrib* is his profession: he had most probably been chosen by the *mukarrib* as his personal contractor or one of his contractors; and his two friends, *⁶mkrb* and *⁶m²ns*, could have been his subcontractors. As for *r²shmw*, his social rank of *mqtwy* made him an important figure in his own clan and, since he is mentioned first in Ir 61, this may suggest that *ṣbḥm* got his job from the *mukarrib* as a result of *r²shmw*'s intervention.

Ir 54: Garbini, p. 31; al-Iryânî, Ar p. 67; above Ir 44; pl. 13, upper center; pl. 38, upper part of the penult right section; pl. 46, upper center; cf. my tracing above, p. 4; cf. summary above, p. 47.

ṣbḥm/fqḍn/ś²k / *²rydy/⁶m/krb²l*	1	Ṣabḥum, the contractor, has beated up
	2	at ²Arayday with Karib²il
	3	1000.

al-Iryânî does not register the vertical line bordering the left side of the inscription, which is longer than the two upper lines. Furthermore, he also fails to record 1.3.

Ir 61: Garbini, p. 22; al-Iryânî, Ar p. 73; pl. 39a; for some remarks, cf. above, pp. 7 and 12; the location of the text remains unknown (cf. above, p. 8).

r²shmw	1	Ra²shumû,
mqtwn	2	the high official,
dmdbḥm	3	he of (the clan) Madbaḥum.
ṣbḥm/fqḍn	4	Ṣabḥum, the contractor,
dmdbḥm	5	he of (the clan) Madbaḥum.

Garbini's interpretation of *mqtwn dmdbḥm* is fantastic. Not only he states that *mqtwn* "in the determinate state ... is specified by the following word introduced by the pronoun *d*" (and he reiterates his blunder in *fqḍn dmdbḥm*), but he also affirms that *mdbḥm* is "used in the indeterminate form, with something like the effect of a plural form in English." Either *mqtwn* or *fqḍn* manifestly is an incidental nominal clause and, therefore, *d* of *dmdbḥm* refers to the author of each section. Garbini crowns his act of imaginary juggling by translating the clan name *mdbḥm* as "sacrifices." Finally, he claims that the text "is slightly separated from the others." What are the sigla of "the others"? Why does he not simply say where the text is located? Ir 60 (pl. 42) is in the right center of pl. 14; and Ir 62 (pl. 41) is in the center of the same pl. 14. Then, Ir 61 could be somewhere in the central right half of pl. 14; but where?

Ir 62: Garbini, p. 28; al-Iryânî, Ar p. 74; below and to the right of Ir 46b, and below and to the left of Ga 95; pl. 14, in the lower half of the central

section; pl. 38, in the lower half of the second group from the left; pl. 41, long, horizontal, central line; for one remark, cf. above, p. 7.

ʿmkrb/fqḍn/ḏʿbn/[w]ʿmʾns/fqḍn/bn/mlʾkm

ʿAmmkarib, the contractor, he of (the clan) ʿAban, [and] ʿAmmʾanas, the contractor, son of Malʾakum.

al-Iryânî omits n of the first fqḍn in his transliteration. Garbini endorses the lettering as seen on the chalked photo, viz. "fqḍnṭn," which he understands as "fqḍ nṭn," and he wastes five lines (pp. 28-29) to explain his erroneous reading. al-Iryânî still retains "ṭn" in his translation, but has "ϟ□" in his transliteration in SA letters, which he correctly interprets as bn son of. It would be a real waste of time to discuss Garbini's imaginary commentary on his "fqḍ nṭn." Finally, both Garbini and al-Iryânî translate ḏ+ʿbn as a single word, "flood" and "as-sayl," respectively.

ṣbḥm/fqḍn of Ir 54 and 61 is a few generations older than his homonym ṣbḥm/fqḍn, son of yṯʿkrb, of CIH 955+418 (cf. M. Höfner, Inschriften aus Ṣirwaḥ, Ḥaūlan (II, Teil), Vienna, 1976, pp. 8-12) because the latter was exercising his profession of fqḍ during three mukarribs, yṯʿʾmr, ydʿʾl and smhʿly, who are mentioned in generations (10) and (12) of the genealogical chart of JaMAR, X, pl. 2. The name of the clan to which ṣbḥm of CIH 955+418 belonged is read as "MD[M]RN" by Höfner (p. 11). However, Gl A 92 = CIH 955 shows that the 3rd letter has a vertical stroke on the left and a tiny vertical stroke on the right, immediately above the horizontal axis of the letter height. It also shows that the top of the letter is missing as well as the two right corners. The most important feature is that the central section of the letter is intact and does not show any trace of the two short aslant lines typical of the center of m; the letter cannot but be b; thus, mdb]⌈r⌉n Madbarân. For the etymology of fqḍ, cf. Höfner, l.c., p. 9, where she retains the word "'Aufseher'" in her translation. The context of CIH 955+418, however, speaks of the building and the repairing of passes and roads, rather suggests the meaning of "contractor" as more probable. Furthermore, the same text CIH 955+418 presents the case of a father and his son being contractors. Thus, fqḍ may be considered as the name of a profession, not of a "social class," as in SDDS, p. 45, where "title" is also suggested. This is vague enough to be correct, but the translation of "title" as "titre de nature sociale" by Ryckmans is inaccurate.

d - brhmw's family.

Ir 51 was engraved by a man called brhmw, and his son, ʿmšfq, is responsible for two more texts, Ir 52 and 53. This is the second family outside the mukarrib's own family which participated in the latter's hunting parties. There is, however,

no clue in the texts as to the reason of brhmw's selection to beat up with the mukarrib, and most of all as to the reason for his son ʿmšfq to be the only outsider of the mukarrib's family to śʾk/wṣyd (cf. below). The only other person who śʾk/wṣyd with the mukarrib is the latter's son yqhmlk in Ir 55 b/1, and not even one of the mukarrib's three brothers (Ir 56b, 57 and 63) did it. Why was such a singular distinction bestowed upon ʿmšfq? We will most probably never know.

Ir 51: Garbini, p. 23; al-Iryânî, Ar p. 65; immediately to the right of Ir 50; general area in pl. 11b, left and pl. 12; pl. 39b, upper left; for some remarks, cf. above, p. 7; for the question of NA, cf. above, pp. 32-33; cf. summary above, p. 41.

ʾlt/śʾk/brhmw[/]	←	1	These has beaten up Barhumû,
bn/gdnm/bʿm/	→	2	of (the clan) Gadnum, with
krbʾl/wtr/	←	3	Karibʾil Watar
dnm/wʾry	→	4	at Danam and ʾAray-
(dy) 𐩹𐩺	←	5	(day:) 150.

L. 1: Garbini refers brhmw of this text to brhmw/bn/ṣbḥhmw/bn/gdnm of Ja 541/8-9, and concludes that "the names brhmw, rʾshmw and ṣbḥhmw, possibly connected with polyandry, are to be noted." How is it possible for anyone in his right mind to even think that the qtlhmw nominal derivation is a possible sign of polyandry? It all obviously started again with br of brhmw (cf. above, pp. 19-21). But it takes quite a simplistic mind to extend the idea of the first name to the others on the sole basis of the nominal derivation. Because of an unknown reason, the photo cut off the left half of the last letter of both ll. 1 and 2. The word divider must be restored at the end of l.1 because it does exist at the end of ll. 2 and 3.

L.2: Garbini erroneously states that, by using the preposition ʿm = bʿm, "the author of the inscription indicates that he belongs to the entourage of one of the two mukarribs." Such a statement is incomprehensible. Since the author of each text had to introduce the mukarrib's name with a preposition, since he used the ordinary, well-known ʿm = bʿm, there is no justification of ascribing to it a special value; and the identity cards, e.g., of the persons belonging to the ṣbḥm's group clearly indicate that they were not a part of "the entourage" of either mukarrib. Although Garbini does not explicit himself on the question, it seems certain that it was his preceding theory that generated that of "the participation of all the court" (cf. above, pp. 40-43).

Most of the participants, 10 cases out of 16, were consanguineous relatives of the two mukarribs; and beside them, three women were maidservants of yṯʿʾmr/byn (Ir 46b and 43 without ʿm/bʿm). These facts do not justify supplement-

ing the texts which do not have any information on the subject. The most that can be said is the obvious, viz. the mukarribs knew their guests well and had good reasons for inviting them. This is illustrated by the case of ṣbḥm (Ir 54) who was most probably invited because of his being a contractor (cf. above, p. 73), a profession which does not justify to count him in the mukarrib's "entourage" which by definition means the attendants. And if a contractor is counted among the mukarrib's subordinates (or, attendants), then the whole nation should be counted also as the mukarrib's "entourage." There is no clue in Ir 57 which might even remotely suggest the reason for brhmw being among the guests. The case of dmryfᶜ with yṯᶜ'mr/byn (Ir 45) is the same as brhmw's with krb'l/wtr. Yet, Garbini's commentary in p. 34 has no cross-reference to his remark on Ir 51. The case of brhmw's son, ᶜmšfq, is remarkable: he is the only alien to the mukarrib's family who fully participated in the two phases of the hunt; cf. ś'k/wṣyd of Ir 53/1 and partially restored in Ir 52 b/1 (cf. above, p. 75). Finally, it should be noted that the prepositions ᶜm = bᶜm are used in almost equal numbers and introduce almost indifferently the first half of the mukarrib's name or his full name, as shown by the following chart:

Prep.	yṯᶜ'mr	yṯᶜ'mr/byn	krb'l	krb'l/wtr
ᶜm	Ir 45/1, 59/2	Ir 44/2	Ir 54/2, 56 b/2, 59/3	Ir 57/1, 58/2
bᶜm		Ir 46 b/3 (+ mr-'hn), 60/2, [64/1], [Ga 97 b/1]	Ir 50/3, 55 b/3,	Ir 51/2, 52 b/4 53/3, 60/3

L.4: Garbini has "'ry[d]y" while al-Iryânî reads "'rydy." The photo does not leave any choice; it is 'ry, and y is the last letter because it is perfectly in line with both k of l.3 and the last m of l. 2. Below ry of 'ry, there are traces of two letters which are dy.

Ir 52: Garbini, p. 24; al-Iryânî, Ar p. 66; below Ir 50 and 51; general area in pl. 11b, left and pl. 12; pl. 39b, lower half; for some remarks, cf. above, p.6; for the division into two texts, cf. above, pp. 10 and 54; for the summary of text b, cf. above, p. 41.

a ...]d/ᶜmšf[q...
b 1 'lt/[ś]ₗ'ₗk[/wṣyd/ᶜ] ←
2 [mš]f(q)/bn/brhmw →
3 [b]n/gdnm/dnm/w'[ry] ←
4 [d]y/bᶜm/krb'l/wtr/ →

a ...]. ᶜAmmšaf[aq ...
b 1 These has beat[en up and hunted ᶜA-]
2 [mmša]fa(q), son of Barhumû;
3 [o]f (the clan) Gadnum, at Danam and 'A[ray-]
4 [d]ay with Karib'il Watar.

Here again, the photo cut off the left end of b/3-4. Ir 52 is a good example illustrating the pitiful condition of the publication of the al-ᶜAqûl inscriptions. The poor photo of pl. 39b makes it impossible to solve many problems raised by these two texts and that especially that of their mutual relation.

Text a - Garbinis states that "a y can possibly be made up before d." This text can hardly have the formula ś'k/wṣyd because not a single text with that formula is composed of one line only.

Text b - L.1: after his "ś'[k," Garbini puts five dots which, however, disappear in his translation; and this shows that, in his opinion, nothing is missing. [/ṣyd/ᶜ], the verb ṣyd being also found in Ir 53, should be restored at the end of the line in order to give the same length to the two lines, viz. 11 letters each. Without that restoration and especially with Garbini's restoration of "ᶜmš" at the beginning of l.2, the length of l.1 would be much too short compared with that of l.2, viz. 6 and 12 letters, respectively. Finally, al-Iryânî reads "ṣd" at the beginning of l.1, contrary to ṣyd of Ir 53. - L.3: according to Garbini, "the word bn is dextrorse." But only the letter n is dextrograde, as in al-Iryânî (Ar p. 66). Compare the wide space between bn and / with that between g and dnm of the same line. Garbini also claims that d of dnm "has a peculiar form" without describing it. His remark is based on de Maigret's chalking which is valueless. Moreover, the end of Garbini's commentary reads as follows:

" It is less clear why 'mšfq wrote two inscriptions rather than just one."

The other inscription, to which he refers four lines above, is Ir 53. The obvious reason for an individual to engrave two hunting records having each its own formula is that he participated in two hunts; there is nothing to explain. Finally, Garbini states that the number of the tally has "disappeared" (cf. above, p. 54). As it stands, his testimony is valueless because Garbini never saw the rock. The tally is not visible in pl. 39b.

Ir 53: Garbini, p. 32; al-Iryânî, Ar p. 67; immediately below and to the right of Ir 44 and below the end of Ir 63; pl. 13, lower part of the central section; pl. 38, lower part of the right penult section; pl. 46, lower right corner; cf. my tracing above, p. 4; for one remark, cf. above, p. 6; cf. summary above, p. 41; for ᶜmšfq's case, cf. above, p. 76.

ing the texts which do not have any information on the subject. The most that can be said is the obvious, viz. the mukarrib knew their guests well and had good reasons for inviting them. This is illustrated by the case of ġbḏm (Ir 54) who was most probably invited because of his being a contractor (cf. above, p. 73), a profession which does not justify to count him in the mukarrib's "entourage" which by definition means the attendants. And if a contractor is counted among the mukarrib's subordinates (or, attendants), then the whole nation should be counted also as the mukarrib's "entourage." There is no clue in Ir 57 which might even remotely suggest the reason for brhmw being among the guests. The case of dmryfᶜ with ytᶜ²mr/byn (Ir 45) is the same as brhmw's with krb²l/wtr. Yet, Garbini's commentary in p. 34 has no cross-reference to his remark on Ir 51. The case of brhmw's son, ᶜmšfq, is remarkable: he is the only alien to the mukarrib's family who fully participated in the two phases of the hunt; cf. ś²k/wqyd of Ir 53/1 and partially restored in Ir 52 b/1 (cf. above, p. 75). Finally, it should be noted that the prepositions ᶜm = bᶜm are used in almost equal numbers and introduce almost indifferently the first half of the mukarrib's name or his full name, as shown by the following chart:

Prep.	ytᶜ²mr	ytᶜ²mr/byn	krb²l	krb²l/wtr
ᶜm	Ir 45/1, 59/2	Ir 44/2	Ir 54/2,56 b/2, 59/3	Ir 57/1, 58/2
bᶜm		Ir 46 b/3(+ mr-²hn), 60/2, [64/1], [Ca 97 b/1]	Ir 50/3,55 b/3,	Ir 51/2, 52 b/4 53/3, 60/3

L.4: Garbini has "²ry[d]y" while al-Iryânî reads "²rydy." The photo does not leave any choice; it is ²ry, and y is the last letter because it is perfectly in line with both k of l.3 and the last m of l. 2. Below ry of ²ry, there are traces of two letters which are dy.

Ir 52: Garbini, p. 24; al-Iryânî, Ar p. 66; below Ir 50 and 51; general area in pl. 11b, left and pl. 12; pl. 39b, lower half; for some remarks, cf. above, p.6; for the division into two texts, cf. above, p. 41.

a ...]d/ᶜmšf[q...

b 1 ²lt/[ś]ₜ²ₖ[/wqyd/ᶜ] ←

2 [mš]f(q)/bn/brhmw →

3 [b]n/gdnm/dnm/w²[ry] ←

4 [d]y/bᶜm/krb²l/wtr/ →

a ...]. ᶜAmmšaf[aq ...

b 1 These has beat[en up and hunted ᶜA-]

2 [mmša]fa(q), son of Barhumû;

3 [o]f (the clan) Gadnum, at Danam and ²A[ray-]

4 [d]ay with Karib²il Watar.

Here again, the photo cut off the left end of b/3-4. Ir 52 is a good example illustrating the pitiful condition of the publication of the al-ᶜAqûl inscriptions. The poor photo of pl. 39b makes it impossible to solve many problems raised by these two texts and that especially that of their mutual relation.

Text a - Garbinis states that "a y can possibly be made up before d." This text can hardly have the formula ś²k/wqyd because not a single text with that formula is composed of one line only.

Text b - L.1: after his "ś²[k," Garbini puts five dots which, however, disappear in his translation; and this shows that, in his opinion, nothing is missing. [/qyd/ᶜ], the verb qyd being also found in Ir 53, should be restored at the end of the line in order to give the same length to the two lines, viz. 11 letters each. Without that restoration and especially with Garbini's restoration of "ᶜmš" at the beginning of 1.2, the length of 1.1 would be much too short compared with that of 1.2, viz. 6 and 12 letters, respectively. Finally, al-Iryânî reads "qd" at the beginning of 1.1, contrary to qyd of Ir 53. - L.3: according to Garbini, "the word bn is dextrorse." But only the letter n is dextrograde, as in al-Iryânî (Ar p. 66). Compare the wide space between bn and / with that between g and dnm of the same line. Garbini also claims that d of dnm "has a peculiar form" without describing it. His remark is based on de Maigret's chalking which is valueless. Moreover, the end of Garbini's commentary reads as follows:

" It is less clear why 'mšfq wrote two inscriptions rather than just one."

The other inscription, to which he refers four lines above, is Ir 53. The obvious reason for an individual to engrave two hunting records having each its own formula is that he participated in two hunts; there is nothing to explain. Finally, Garbini states that the number of the tally has "disappeared" (cf. above, p. 54). As it stands, his testimony is valueless because Garbini never saw the rock. The tally is not visible in pl. 39b.

Ir 53: Garbini, p. 32; al-Iryânî, Ar p. 67; immediately below and to the right of Ir 44 and below the end of Ir 63; pl. 13, lower part of the central section; pl. 38, lower part of the right penult section; pl. 46, lower right corner; cf. my tracing above, p. 4; for one remark, cf. above, p. 6; cf. summary above, p. 41; for ᶜmšfq's case, cf. above, p. 76.

1	ʾlt/śʾk/wṣyd/ʿmšfq
2	bn[/]brhmw/bn/gdnm/dnm
3	wʾrydy/bʿm/krbʾl
4	wtr

1 These has beaten up and hunted ʿAmmšafaq,
2 son of Barhumû, of (the clan) Gadnum, at Danam
3 and ʾArayday with Karibʾil
4 Watar: 1000.

al-Iryânî does not register the long vertical line to the right of ll. 1-3; erroneously registers a word divider after bn of l.2; assigns a special line to the number of the tally; and reverses the ductus of the initial ʾ; for the number at the extreme left of his text, cf. the commentary on Ir 44 (cf. above, p. 51). Garbini does not note the dextrograde ductus of the initial ʾ.

2 - yṯʿʾmr/byn's hunts.

The two characteristics of yṯʿʾmr/byn's hunting parties are the presence of three women among the five participants and the absence of any tally of killed animals.

a - The beater maidservant gḥmt (Ir 43).

The woman gḥmt belonged to the mukarrib's household through the relation of maidservantship. She must have been quite an unusual woman for participating in the beating up of the beasts during one of her master's hunts; but her tally is the lowest among all the figures recorded (cf. below, the tallies; p. 83).

Ir 43: Garbini, pp. 30-31; al-Iryânî, Ar pp. 57-58; immediately below Ir 42; about 2/3 of the right section in pl. 13, lower left of the left panel; pl. 38, lower half of the right penult of the right series; pl. 45, lower half; for some remarks, cf. above, p.6; cf. summary, above, p. 41.

ʾlt/śʾkt/gḥmt	1	These has beaten up Gaḥmat,
ḏt/byt/yṯʿʾmr	2	she of the house of Yaṯaʿʾamar,
ʾmthw/ʾrydy/wdnm	3	his maidservant, at ʾArayday and Danam:
oo	4	20.

The two characteristics of the text are the irregular engraving and the small height of t. The letters ʾm (l.3) are engraved astride a long crack, but thw are above that crevice. al-Iryânî does not register the number in l.4, but Garbini reads "20," viz. two large circles partially visible under thw.

Garbini does apparently not know how to handle the well-known word ʾmt since he renders it as "bride" in his formal translation of the text. Then the word becomes "with lesser probability, ʾṯthw ... a wife of the first rank, rather than the second" (p. 31). It does not matter that he does not know whether there were wives of

the first, second, etc, rank. Then, he deliberately chooses his second reading, viz. that "with lesser probability:" he speaks of "a wife of the sovereign" (p. 31; cf. also p. 22), and in the commentary on Ir 42 (p. 30) he refers to her as "to the sovereign's wife, or favourite as she may have been" (for the quote of his text, cf. above, p. 37). In fact, the reading of "ʾṯthw" is disproved by pl. 13 where the second letter appears as]; thus, "ṯ" is out of the question. Actually, as already stated above (p. 78), the expression ḏt/byt qualifying the woman's name indicates that she was a member of the mukarrib's household, but not by birthright, and her social condition is given by her title as ʾmt maidservant. It is ridiculous to interpret ḏt/byt, as Garbini does (p. 31), as suggesting that "polyandry may have been customary" with reference to W.W. Müller's theory on polyandry, which is discarded by my Carnegie..., pp. 162-164. Moreover, Garbini states that the name of the woman, gḥmt, is "found, also in the feminine, in North-Arabic," and as usual, he does not give any reference. Garbini blindly copied G.L. Harding, An Index and Concordance of the Pre-Islamic Arabian Names and Inscriptions, Toronto, 1971, p. 154: "JS 364/3." Of course, Garbini did not notice that Harding also refers to the very same text as "uncertain" under "jḏmt" (p. 157), and he simplistically treats that Liḥyanite name as feminine because of the -t ending. In fact, the siglum "JsaL 364" contains two different texts, viz. ll. 1-2 = a = Minaean (cf. my Minaean Inscriptions Published as Liḥyanite, Washington, 1968, pp. 52-53) and l.3 = b = Liḥyanite. Of course, again, Garbini does not know that the editors (cf. A. Jaussen et R. Savignac, Mission archéologique en Arabie, Paris, II, 1914, p.528) had already read the second consonant of the name as "s," and that W. Caskel (cf. Lihyan und Lihyanisch, Cologne, 1954, p. 146A) had already given a quarter of a century ago the correct reading, gsmt, to whom Harding paid no attention. JsaL 364b reads ln gsmt Gasmat was treated gently. - ln, cf. Thamudic Ḥâʾil 101. - gsmt is a masculine name.

b - Two more maidservants (Ir 46b).

Ir 46: Garbini, p. 25; al-Iryânî, Ar p. 60; pl. 14, extreme upper left; pl. 38, upper section of the extreme left panel; pl. 40b; pl. 41, upper extreme left; for the division of the siglum into two texts, cf. above, pp. 9-10; for some remarks, cf. above, pp. 6 and 14; for the summary, cf. above, p. 41.

khlm	a		Kahilum.
wšʾbt/w.rʿ.[/ʾ]	b	1	Wašʾabat and .rʿ. [, maid-]
mh/yṯʿʾmr[/byn]		2	servants of Yaṯaʿʾamar [Bayyin,]
bʿm/mrʾhn		3	with their [= the women's] lord
dnm/wʾr[ydy]		4	at Danam and ʾA[ayday.]

Text a - al-Iryânî clearly separates hlkm from the remaining lines. There is no way of knowing the role played by hlkm in the mukarribs' hunts, if any.

Text b - al-Iryânî registers "mhw" at the beginning of l.2 although there is no room for the letter w. What he reads as "w" is a small feature in the stone immediately to the left of the lower half of h. Garbini's wild reading and commentary are unworthy of discussion.

L.1: wšʾbt, a qtʾlt nominal derivation; cf., e.g., a qtʾlm form in ḥdʾbm in JaSIWD, p. 419A. - L.2: the restoration of byn at the end of the line is justified to give the same number of letters, 11, to both ll. 1 and 2. - L.3: mrʾ refers to the mukarrib mentioned in l.2. - Ll. 3 and 4 also have almost the same number of letters, 8 and 9, respectively. The restoration of "dy" instead of "ydy" is disproved by the absence of the defective writing of the name (cf. above, p. 13).

The story of Ir 46b parallels and confirms that of Ir 58 and 68 with ʾdbʿly and drḥn, respectively (cf. above, p. 72). The two maidservants of yṯʿʾmr/byn did not take any active part in any of their master's hunts; yet, they went at least once to a resort located in the general area of the two hunting grounds which they must have visited since they mention the names dnm and ʾr[ydy]. Therefore, that resort must have been some kind of country lodge to which their master occasionally retired at leisure. The case of krbʾl/wtr's young grandson ʾdbʿly is similar to the preceding: he too did not take part in any hunt but he was at least once in the vicinity of the hunting grounds since his text is engraved at al-Jafnat.

c - The beater dmryfʿ (Ir 45).

The beater dmryfʿ engraved his text in an isolated place of unknown location (cf. above, p. 8).

Ir 45: Garbini, p. 34; al-Iryânî, Ar p. 59; pl. 48c; cf. summary above, p. 41.

ʾlt/śʾk/dmryfʿ/ʿm/	1	These has beaten up Damaryafaʿ with
yṯʿʾmr/dnm/wʾrydy	2	Yaṯaʿʾamar at Danam and ʾArayday:
byn	3	Bayyin
𐩹𐩺	4	150.

In al-Iryânî's siglum, correct "22" to "27." al-Iryânî correctly reproduces the text including the dextrograde n of byn. But Garbini integrates l.3 into his l.2 and, then he invents a nonexistent word divider. It is obvious that the engraver had forgotten byn in l.2; he added it immediately below yṯʿʾmr.

d - The beater dr... (Ir 64).

The text is so fragmentary that it allows us to know only that it was engraved by yet another beater of one of yṯʿʾmr/byn's hunting parties.

Ir 64: Garbini, p. 34; al-Iryânî, Ar p. 75; pl. 48b; for some remarks, cf. above, pp. 7 and 54; the location remains unknown, cf. above, p. 8; cf. summary above, p. 41.

ʾlt/śʾk/dr[.../bʿm/yṯʿʾmr]	1	These has beaten up Dar[... with Yaṯaʿʾamar]
byn/𐩹𐩺/	2	Bayyin: 25.

Both Garbini's and al-Iryânî's transliterations are limited to the chalked letters of pl. 48c. Garbini states that "the formula used in this inscription was different from the others." His reasoning is wrong: "ṣ (possibly to be read ṣy...);" and this mere possibility becomes a probability in the very next line: "the verb ṣyd was probably present." Garbini does not even see the difference between possibility and probability. His possible or probable "y" is unjustified because the traces to the left of "ṣ" are two short straight lines making a V contrasting with the round top of y. al-Iryânî's transcription of the end of l.2 is "ṣ..."

: this is the photocopy of the chalked letters. I suggest completing the initial "ṣ" to ṯ; the level of the bottom of the right word divider shows that there is plenty of space for the bottom of ṯ. Note also that the chalking of "ṯ" as an error instead of ṣ is found in Ir 42/3 (pl. 45). Then, the 2nd letter is easily completed as h; and the number becomes 25, not "26" because the last vertical bar on the left is the word divider corresponding to the other on the right. The letter ṯ with the value of 20 is attested in RÉS 2965/4. The SA number of RÉS 2965/4 is 𐩲𐩺𐩹𐩺 = 180, as indicated by the two immediately preceding words, ṯhmnhy/wmʾt; the 𐩺 is perfectly clear in Gl A 258c. Thus, the value of the hapax legomenon as 20 is ascertained. J. Halévy's copy shows " 𐩲𐩺·𐩺 " (cf. JA, 19 [1872], p. 222, no. 466), and the second element, 𐩹, was already restored by F. Hommel in Süd-arabische Chrestomathie, Munich, 1893, p. 105, who, however, transforms the 3rd element 𐩺 to " 𐩺 ;" thus, "𐩲𐩺𐩹𐩺." Hommel does unfortunately not make any remark on the number; he, nonetheless, eliminates the first evidence of 𐩺 = ṯ = 20. He probably thought that the crossbar between the two circles in Halévy's copy was an error. As stated above, that crossbar could not possibly be clearer in Glaser squeeze. By writing two circles one on top of the other, Hommel paved the way to RÉS (V [Oct. 1929], pp. 264 and 265) who claims to transcribe "Texte d'après Hommel" and transforms the original number by giving it five figures, "mʿʿʿʿ" = 𐩲𐩲𐩲𐩲𐩺, instead of four; yet he still gives it the value of "180." Then, K. Conti Rossini reaffirms Hommel's copy, " 𐩲𐩺𐩹𐩺 " (cf. Chrestomathia...., p. 84, no. 79/4). In 1968, RÉS, VIII, maintains RÉS's five figures, but corrects the first 𐩲 to 𐩹 so that the number be "180" (pp. 189A and 263), and still fails to

list the value of "20" under t (p. 252A). In 1974 (cf. Iscrizioni minee, Naples, p. 54, no. 185), A. Capuzzi introduces the worst mistreatment of the number by writing "80" with reference to the explicative note 11 where she claims: "Lettura: Halévy, Inscriptions sabéennes, p. 222, n. 466." Halévy's copy (cf. above), ○8·8, can most certainly not be "80." In spite of Halévy's 8 which she apparently did not check against Gl A 258c, M. Höfner does not list the number in any of her two tables (cf. Atsüdarabische Grammatik, Leipzig, 1943, pp. 13-14), which are faithfully reproduced by G.M. Bauer (Yezik uzhnoaraviskoe pismennosti, Moscow, 1966, p. 28). If the origin of the number t does not make any difficulty, its very rare usage does, viz. why was it not used, e.g., in RÉS 2771/6, 2778/10, etc? At least, its value is certain. The present general mentality of SA research being what it is, it is highly probable that the engraver of RÉS 2965/4 will be accused of an error by some enlightened writer.

Another forgotten SA number is 𐩺 of RÉS 2919/2 of which J. Halévy's copy (cf. JA, l.c., p. 212, no. 413) is also definitely confirmed by Gl A 414a, where it could not be any clearer, but the text does not give any clue as to the value of that number. RÉS (V, p. 236) equals it to "ʿ" = "10 (?)," an obvious error which is repeated by A. Capuzzi (cf. Iscrizioni..., p. 42, no. 141). This figure is not mentioned by M. Höfner (cf. l.c., and Bauer, l.c.) and RÉS (VIII, p. 229, under ṣ).

Finally, it is quite understandable that none of A.F.L. Beeston's two grammars (1962 and 1984) contains any presentation or mention of the SA numbers due to the impossibility of indulging with them in any kind of pan-arabism or in any cerebral or verbal hairsplitting on morphological or syntactical ground.

e - Another unnamed beater (Ga 97b).

The siglum Ga 97 covers two texts (cf. above, pp. 9-10). It is very doubtful whether text a belongs to the series commemorating one of the two mukarribs' hunting parties. Text b is even more fragmentary than Ir 64.

Ga 97: Garbini, p. 33; pl. 47c; not dealt with by al-Iryânî; for some remarks, cf. above, p. 7; the location of the text remains unknown, cf. above, p. 8; for the question of the end of text b and the division of the siglum, cf. above, p. 9.

a	b	
...]ʿf(l)/rnt	ʾlt[/śʾk/.../bʿm/y]	1
	tʿ[ʾmr/byn/ʾr]	2
	˹y˺dy	3
	w[dnm]	4

a: ...]ʿf(al of the clan) Ranat.

b: 1 These [has beaten up ... with Ya-]

2 taʿ[ʾamar Bayyin at ʾAr-]

3 ayday

4 and [Danam].

Garbini's readings and commentary are useless.

Text a - rnt, cf., e.g., the personal name rntm in CIH 840/1, and feminine in CIH 495/3-4.

Text b - The tally is located at the end of 1.4 or more probably in 1.5 below 1.4.

F - The tallies of the hunting parties.

As mentioned above (p. 54), each text beginning with ʾlt ends up with a tally, which is unavailable in Ir 50, 52b and Ga 97b. In other inscriptions, a tally may be given, as in krbʾl/wtr's Ir 49 and in ṣbḥm's Ir 54/3 (no ʾlt, but with śʾk). It may, however, be omitted as in the two texts engraved by ytʿʾmr/byn, Ir 41 and 42. Of course, the verb śʾk is not found in any mukarrib's text. Here follows the list of the tallies known so far, which complements the schematic presentation of the texts (cf. above, p. 41).

ytʿʾmr/byn beat up	ytʿʾmr/byn kill	Tallies (total: 3755)	krbʾl/wtr beat up	krbʾl/wtr kill
Ir 43/4		○○ 20		
Ir 64/2		⅄8 25		
Ir 44/3, 60/3		¶ 50	Ir 56 b/3, 60/3	
		8 100		Ir 49/2
Ir 45/4		¶8 150	Ir 51/5	
		88 200	Ir 57/2, 59/4	
Ir 59/3		888 300		
		○¶8888 460		Ir 55 b/3
		⅄ 1000	Ir 54/3	Ir 53/4

The preceding figures do not show any sign of competition either between the two mukarribs or among the hunters of each group. It is indeed difficult to see the "de facto predominance of Karibʾil" claimed by Garbini (cf. above, pp. 46-48) because neither the two texts engraved by ytʿʾmr/byn, Ir 41 and 42, nor any inscription of his followers give any figure of killed animals. It is impossible to determine the "de facto predominance of Karibʾil" as long as the comparison with the tallies of the other party remains unfeasible. Furthermore, krbʾl/wtr's "de facto predominance" is contradicted even by his own parties because he killed 4.6 times less than his own son (Ir 55 b/3) and even 10 times less than his guest

ʿmšfg (Ir 53/4).

From our viewpoint it is regrettable that no text indicates what kind of wild animals was hunted. Obviously enough, the figures of the tallies are approximations since no unit, |, is attested so far and numbers, such as "1000," can hardly be strictly accurate.

The location of the number of the tally always is at the end of the record of the hunting party. Therefore, when a text deals with two hunts, the first tally is between two word dividers and precedes the record of the second hunt, as in Ir 59/3. In all other texts, which mention each one hunt only, the tally is found in several positions:

a - immediately after the end of the text:

1 - with a word divider or a line after the number, as in Ir 49/2, 57/2 and 64/2; or

2 - without any following line or word divider, as in Ir 55 b/3;

b - isolated after the end of the last line:

1 - between two markers, as in Ir 53/4;

2 - no word divider and no marker, as in Ir 51/5;

c - isolated in a special line:

1 - between two word dividers, as in Ir 59/4;

2 - between two markers, as in Ir 54/3 and 56 b/3; and

3 - no marker and no word divider, as in Ir 43/4, 44/3 and 45/4.

G - Texts non belonging to the mukarribs' hunts.

Among the sigla containing two texts each, Ir 52a and Ga 97a do not belong to the collection of the two mukarribs' hunts, and it remains doubtful that Ir 46a belongs to that series of inscriptions. Furthermore, Ga 96 is excluded because no photograph is available. Finally, palaeography indicates that the authors of the 12 following texts were not of the type of persons who participated in the mukarribs' hunting parties, viz. from al-ʿAqûl: Ir 65, Ga 95, 98 and Ja 3208; and

from al-Jafnat: Ir 66a, b, 67a, b, Ga 99, 100, 101 and 102;

for the location of these texts, cf. above, pp. 11-12.

All those 12 texts have in common that they are not studied by al-Iryânî.

1 - Ir 65: Garbini, p. 25; pl. 40a; cf. above, pp. 7 and 8, for the size of the letters and the unknown location.

lḫyṯt/bn/ġ	1	Laḥayṯat, son of Ġa-
lftn/yhrʿd	2	lfatân, of (the clan) Yuharʿid.

lḫyṯt: Garbini understands the name as an example of the aphaeresis of ʿ of ʿṯṯ, as in JaSIMB, p. 412B, in the case of šrḫtt. A.P.L. Beeston speaks of ʾ, ʿ and ḥ as the

"back-of-the-throat sounds" (cf. Sabaic Grammar, Manchester, 1984, p. 10, par 2:4), which are commonly referred to as laryngals; but he erroneously speaks of ʿ as "the velar" (cf. CIASA, II, p. 120). He also claims (cf. Sabaic, l.c.) that the "cases where ʾb '(my) father' and ʾl '(my) god' are reduced to b and l in compound personal names, such as J 631/26 l-ʿzz-m" should be understood as ʿṯt > tt. His rendering of both ʾb and ʾl is gratuitous and unjustified because he cannot, and does not even attempt to, prove that any kind of personal personal pronoun is understood; and his understanding is only for the purpose of rendering his opinion more acceptable. His interpretation is further contradicted by a name such as šrḫb ʾl, where b cannot be understood as ʾabu or abî; the letter b is a preposition. Furthermore, Beeston's example lʿzzm, which is also picked up from JaSIMB, l.c., does not prove the existence of the element l understood either as a preposition or as the aphaeresis of ʾl, as it occurs in NA. The names lʿzzm, lʿzm (Gl 1658/3) and lʿzmm (Ja 2666 f) are nominal derivations from the root lʿz; cf. Ar laʿaza "rem habuit cum muliere" (cf. FrLAL, IV, p. 110A). As for the etymology of lḫytt, the Minaean family name stated by K. Mlaker in Die Hierodulenlisten von Maʿîn, Leipzig, 1943, pp. 26 and 37, ytt in Gl 1014/1 (= RÉS 2773/1), and the name of a Sabaean city, tt, attested in Ja 661/5 and 6 (cf. the commentary in JaSIMB, p. 167A), calls for a radical tt. The roots twt and wtt do not exist, and the root twy is excluded by the Minaean name. There remains Ar tatt "Qui cacat mingitve tempore coitus; Fissura in pietra" (cf. FrLAL, I, p. 211A). Finally, due to the extreme rarity of the use of that root in SA onomastics, the chalking must be questioned for accuracy. - ġlftn, cf. Safaitic ġlf in JaS 138a, and Ar ġalafa "to put, wrap (a thing in a cover)" (cf. E. W. Lane, Arabic-English Lexicon, p. 2283). The nominal derivation qtltn is very well-known. Garbini's commentary is a gem of nonsense. - yhrʿd, a Qatabanian family name; cf., e.g., RÉS 3902, no. 56 (cf. already Garbini).

2 - Ir 66: Garbini, p. 35; pl. 49a; for the division of the siglum into two texts, cf. above, pp. 9-10; for one remark, cf. above, p. 7.

ẓwrʾmr/ysrn	a		Ẓawwarʾamar of (the clan) Yasrân.
ḫlḫlm/wklbm	b	1	Ḫalḫalum and Kalbum,
btʿnhn		2	the two Bataʿites.

There is hardly any space between the two texts.

Text a - ẓwrʾmr, cf. the Minaean divine epitheton ẓwrʿdn in RÉS 2743/7. W.W.Müller's suggestion of the personal name "ẓwrwkl" in RÉS 4528 (cf. Die Wurzeln mediae und tertiae Y/W im Altsüdarabischen, Tübingen, 1962, p. 78) is simplis-

tic and improvable; cf. RÉS's interpretation (VII, p. 249): "ṯwrw(/)kl." The text is so fragmentary that the plural verbal form is less acceptable than the singular; thus, ṯwr[/]wkl. Cf. also ṯwrkrb of Ir 44/1 (cf. above, pp. 51-52) and ṯwrʿly of Ir 67a (cf. immediately below). - ysrn, also a second personal name in RÉS 3894/1, but mostly illustrated as a place name near Mârib; cf., e.g., JaSIMB, p. 415B. Text b - blḥlm: very well-known clan name; cf. JaSIMB, p. 414A. - btʿnhn, for the well-known tribe name btʿ, cf., e.g., JaSIMB, p. 413B.

3 - Ir 67: Garbini, pp. 35-36; pl. 49, upper half; for one remark, cf. above, p. 7; for the division of the siglum into two texts, cf. above, pp. 9-10.

ṯwrʿly/ʿlhn/	a		Ẓawwarʿalay of (the family) ʿAlhân.
dʾbm/bn(/g)fśʾl	b	1	Daʾbum, son of (Ga)faśʾil,
yqḥ		2	is recovering.

Text a - ṯwrʿly, same nominal formation as ṯwrʾmr of Ir 66a (cf. immediately above). - ʿlhn, very well-known name of a person and of a family; cf., e.g., JaSIMB, pp. 410B and 416A, respectively.

Text b - dʾbm, e.g., CIH 496/2. - The following name is read by Garbini, who is endorsing the chalked letters, in two different ways, viz.

"bfś3ʾl" in his transliteration (p. 35) and at the beginning of his commentary (p. 36), and

"Rfśʾl" in his translation (p. 35). The name is presented in the commentary (p.36) as "probably North-Arabic (b-fs-ʾl 'in the palm of god'; cf. Phoenician ṗs and Hebrew pas)." Not knowing what to do with the name, Garbini solved his problem by ascribing the name to NA. But as usual he does not specify which language he is referring to (cf. already above, pp. 32-33). In fact, the bqtlʾl nominal derivation does not exist either in SA or in NA; and Phoenician ṗs means "tablet" (cf. R.S. Tombach, A Comparative Semitic Lexicon of the Phoenician and Punic Languages, Missoula, 1977, p.266; and S. Segert, A Grammar of Phoenician and Punic, Munich, 1976, p. 299). It is difficult to accept the chalked tracing because the empty space between the two chalked letters "b" and "f" is much larger than any other. For the integration of a word divider into the following letter and the consequent distortion of the letter, cf. Ir 53/2 and 55 b/3. Therefore, the left vertical staff of that "b" had an appendix: either ǵ, d or l. Here follow the copy of the chalked letters and, immediately below, my restoration. The letter l is excluded because the root "lfs" does not exist. The letter d is also excluded because there is not enough space to the left for a normal-sized appendix of d. In addition, the root "dfs" exists only in the 9th form ʾidfassa

"nigro faciei colore affectus fuit citra morbum" (cf. FrLAL, II, p. 41A); thus, the root dfs is extremely rare. For ǵfs, cf. Ar jafisa "stomacho gravatus fuit eiusque laboravit indigestione" (cf. FrLAL, I, p. 287B). - yqḥ: the root qwḥ conveys the idea of "to restore, complete, set in order." As the text does not mention any building that would have needed such attention, the author must be speaking of himself; thus, the verb may be translated as "is recovering; is getting back to normal." Either expression covers the double possibility that some kind of calamity had affected his wealth or health. By this text, the author gives the signal that he is over it, whatever might have been the case.

4 - Ga 95: Garbini, pp. 27-28; pl. 41, below Ir 49 and above the first half of Ir 62; pl. 14, right half of the right panel; pl. 38, lower half of the second panel from the left; for one remark, cf. above, p. 7; for the question of NA, cf. above, pp. 32-33.

krbm/hll/ghrt[/]dmrś

Karbum has welcomed Gahrat, him of (the clan) Mariś.

Garbini's treatment and commentary of 36 lines exemplify the Garbinian reckless utopia and its constant arrogance which are illustrated by the four following excerpts, viz. (a) he claims that the text is

" in North-Arabic characters of the type known as «Thamudene A» or «Teymanite». This is the earliest phase of North-Arabic script and is documented in the north between the end of the 6th and the 5th centuries B.C." (p. 27);

(b)" The inscription is composed solely of names of personages enjoying great prestige in the court, given that their names appear immediately after those of the sovereigns. They were evidently foreign guests " (p. 28);

(c) in his understanding of his "ghrt dmfś," his feminine name ghrt is the antecedent of the masculine pronoun d which " is to be considered feminine " (p. 28);

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" The use of North-Arabic script and perhaps, if our view is correct, of North-Arabic language" (p. 28).

It should be strongly emphasized from the very start that Garbini refrains from giving his translation of a very simple text composed of four words only.

His 1st statement is a good example of his scientific dishonesty because the letters ǵ, d and ś do manifestly not belong to the so-called "Teymanite," a word which indicates only a geographical relation, not a linguistical characteristic. But his 1st affirmation must be corrected by his 4th: he wants us to believe that the text would be written in ancient Ar of the north, which is nonsensical. Furthermore, both l, on which Garbini remains silent, and m, about which he speaks only of a "variant form," are typically Lihyanite, not Thamudic. It is also noteworthy

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thy to point out that Garbini contradicts himself without knowing it. On the one hand, he claims that those "foreign guests" utilized the "North-Arabic script used in the Yemen" but "adapted to some extent to the South Arabic script" (p. 27). On the other hand, "we have ... the possibility that the North-Arabic script was known in Yemen before the introduction by the Sabaeans of the South-Arabic monumental script" (p. 28). This is in the pure Pirenne spirit (cf. above, pp. 35-36), viz. a juggling upon words based exclusively upon, and aimed at, justifying Pirenne's preconceived house of cards. Where does either "the South Arabic [sic!] script" (p. 27) or "the South-Arabic monumental script" (p. 28) come from? Was it a gift from the SA gods delivered to Saba by some unknown meteorite or by the legendary hoopoe? "The South-Arabic script" mentioned by Garbini started about the 12 century B.C. (cf. my paper in BASOR, No. 172, December, 1973, pp. 41-54) and rapidly became formal, more rationalized, more unified in the so-called monumental alphabet although it remained used by ordinary people as testified by the many cursive texts; a good example of the coexistence of the two letterings is Ja 2224 (cf. JaMAR, II [1971], p. 76 and plate 4), of which the cursive text b still exhibits the , the rounded type of which Pirenne singles out as one of her "lettres aberrantes" = "signes thamoudéens ou locaux" (cf. above, p. 34). It is not the letter that is "aberrante."

His 2nd affirmation is nothing but an application of his 1st statement to his imaginary scenario of the mukarribs' hunts (cf. above, p. 44). This section of his imaginary staging is presented in two phases which, although, complementary to each other, are not delivered in the same flash of his imagination; they are separated from each other by the equivalent of almost five lines of his text.

His 1st gush is that the text "refers to two personages, a man and a woman, who are apparently not linked by family ties" but only by "some relationship" (p. 28). The reader feels frustrated by Garbini's silence on that "relationship" because he feels cheated of a section of an unusual story. He also regrets that Garbini fails to justify (1) the absence of the conjunction w and between the two names; (2) the presence in a NA text of an hapax legomenon, viz. a personal name composed of two separate elements; and (3) the absence of any clan affiliation after the masculine name contrasting with that after his feminine name. Of course, Garbini is deeply convinced that the shining glare of his imaginary mastership is accompanied by the blanket of a dark shadow the nature of which is to swallow to oblivion the obvious preceding facts.

His 2nd gush speaks of the male "person who seems to be ethnically Qatabanian but is in the company of a North-Arabian woman (who may have materially executed the epigraph)" (p. 28).

Here again, the reader feels frustrated by Garbini's silence on his reason justifying

his "ethnically Qatabanian" for the man because, here again, he is deprived of another element of a fairy tale. The name krbm is equally attested in both Sabaean and Qatabanian; and the name hll belongs to the Qatabanian dynastic onomastics and is not used by others; but it is attested as Sabaean in Ja 2647b. Therefore, it is quite possible that Garbini bases his erratic affirmation on the Qatabanian dynastic second name!!! Avanzini erroneously copied "hllyhn la double tribu de hll" of Weigall 2 (cf. Glossaire des inscriptions de l'Arabie du Sud, Firenze, II, 1980, p. 289A) from G. Ryckmans in Le Muséon, 62 (1949), p. 57, where the 4th letter from the end is between stippled brackets and both the word itself and its translation are followed by a question mark. A. Weigall's facsimile (cf. Travels in the Upper Egyptian Deserts, Edinburgh, 1913, pl. IV, no. 14) clearly shows r, instead of the second "l;" thus, hgryhn the Hagarite. The -hn article and ś instead of t in qśm²l indicate the text as Ḥaḍrami. Finally, here again, Garbini's statement on the woman as the possible engraver of the text is imaginary and completely unfounded. The first named person ordinarily is the engraver or the person who ordered the engraving. I am unable to fathom which feature of the text might have been the starting point of Garbini's twaddle.

His 3rd statement was already anticipated by his two preceding thrusts explicitely mentioning the presence of a woman. The reason for ghrt to be feminine is in all probability the understanding of the -t nominal ending as the proof of the feminine gender, as he already did with a Liḥyanite text (cf. above, p. 79). Thus, Garbini's whole scheme is in shambles, and to complete the inanity of his theory, it suffices to say that the fourth name of the text could easily be understood as a dqtl nominal derivation which is well attested in both SA and NA.

His 4th affirmation is the conclusion of an imaginary staging based on an incompetent approach to a very simple text.

krbm, e.g., RÉS 2726/21, and Qatabanian Ja 1076/1. - hll, cf. Ar halla "souhaiter la bienvenue à qqun" (cf. C. de Landberg, Glossaire de la langue des Bédouins ʿAnazeh, Uppsala, 1940, p. 90). - ghrt, cf. the name of the city ghrn in Ja 555/3. - mrś, second personal name in RÉS 5001.

5 - Ga 98: Garbini, p. 33; pl. 48a.

lḥy/fq Laḥay has hiccoughed [here].

fq: this word does not at all look like a family or clan name but appears to be a verbal form; cf. Ar faqa (o) "to hiccough, pant" (cf. H.A. Salmoné, An Advanced Learner's Arabic-English Dictionary, Beirut, 1972, p. 730B).

6 - Ja 3208: Garbini, introduction to Ir 55 (p. 33); pl. 48a and 47c, where about the right half of the upper

section was cut off by the photo.

My tracing (cf. on the left) was made from pl. 48a, the photo of pl. 47c shows the petroglyth distorted by the aslant photo. The large stippling indicates the right edge of the shaded section of the rock. The petroglyth is engraved to the right of Ir 55b and to the left of Ga 98, on the other side of the large cleft. The whole panel is at an obtuse angle with that bearing Ir 55b.

? ?
ydḫ Yadaḫḫ

Garbini states that

"To the right of the inscription, there is a figure composed of a monogram (formed of the characters *ḏ*, *ḥ* and *y*) surmonted by a horizontal line and a square. This is very probably a magical figure (letter *ḏ* very common in magical contexts, plus the word «life», *ḥyw*: the letter *w* can easily be made out where the upper part of the *ḥ* joins the *ḏ*)."

Sheepishly following into the footsteps of E. Anati (cf. JaMAR, I [1971], pp. 71-81), Garbini sees magic and deity everywhere. His statement on d is preposterous; no less impossible is to read the cupule of ḫ as "w." As a matter of fact, his description of the upper section is grossly erroneous: there is neither "a horizontal line" nor "a square." The value of those two signs remains unknown. The extension of the tail of ḫ below the lower horizontal bar of d indicates that the lowest section of d must not be understood as b; thus, dyḫb is excluded (cf. yḫb, e.g., in Ja 2737 aj). Cf. Ar *ḍaḥḥa* "palma percussit aliquem" (cf. FrLAL, II, p. 81B).

7 - Ga 99: Garbini, p. 35; pl. 49b; on a boulder in front of the panel of Ir 67.

ymʿtt Yawmʿatat.

The name is engraved on a rock in front of the panel with Ir 67 a and b. As usual, Garbini does not give any detail on his analysis of the name. As it stands, it is composed of ym (= ywm) + ʿtt. For the defective writing of ywm as ym, cf., e.g., CIH 338/3. The roots mʿʿ, mwʿ and wmʿ do not yield any satisfactory meaning for a name with -tt as second element, which is extremely rare (cf. above the commentary on lḫytt of Ir 65/1 comp. 85). The question, however, must be raised as to whether or not the initial letter of the name was cut off by the photo because it had not been seen and, therefore, challenged.

8 - Ga 100: Garbini, p. 36; pl. 50a.

yqh 1 Yaquh-
yfʿ 2 yafaʿ.

Garbini reads l. 1 as "yqḫ" in spite of the fact that, even according to himself, the "final character appears to be an h rather than an ḫ" (cf. already above, p. 7). In fact, there is not the slightest trace of any median stroke in the cup of h, which might suggest his reading. It is a new compound name with yqh as the first element. Garbini's commentary contains another example of his argumenting method being made for the sake of the cause. He writes that

"the verbal form *yqh* followed by a proper name makes little sense as the type of inscription justifies neither the sense of «to order» (*wqh* in South-Arabic) nor of «to obey»."

Yet, in the commentary on Ga 101, which immediately follows Ga 100 on his p. 36, he publishes the name yqhmlk borne by an ordinary individual... and Garbini finds no difficulty apparently because the name "is quite common in South-Arabic." Then, he should have had no difficulty in reading the name of the present text as yqhyfʿ. As far as "some magic figures" on the right of the text, viz. a large d "and three circles," they cannot be discussed here without a good photo; note that two of the "three" circles are not visible on the photo. Finally, for Garbini's understanding of d and the circles as "magic figures," cf. above, p. 90.

9 - Ga 101: Garbini, p. 36; pl. 50b; for the question of NA, cf. above, pp. 32-33.

yqhmlk/syḫn Yaquhmalik of (the family) Sayḫân.
Wasm Wasm

yqhmlk, cf. Ir 55 b/1, and above, the commentary on Ga 100. - syḫn, cf. the Thamudic feminine syḫ in HuIR 134. Ph 420 h is published by G. Ryckmans in Le Muséon, 78 (1965), p. 220 and Philby's facsimile in p. 227:
As usual, Philby's facsimile is especially poor and Ryckmans limits himself to this interpretation; "s/ḫl Marques ou initiales. Le l est à l'envers." Were l "à l'envers," it would still be upside-down, viz. . The text is a personal name of which the 2nd letter is d, y or l, and the 4th the upper 2/3 of a normal n; thus, sd/y/-lḫ₁n₃. The most probable reading is s(l)ḫn, a proper name known, e.g., in ʾAwsanite RÉS 3887/1, Ḥaḍrami RÉS 2687/1, Qatabanian RÉS 3902, no. 17, and Sabaean in Jamme SIMB, p. 416B.

10 - Ga 102: Garbini, p. 36; pl. 51a; for the question of NA, cf. above, pp. 32-34.

dmrkrb Damarkarib. Cf. Ir 60/1.

Conclusion

The collection of Sabaean rock inscriptions published by IsMEO and studied above relates succinctly an undetermined number of hunting parties sponsored by the two mukarribs yṯʿʾmr/byn and krbʾl/wtr, the first enjoying the incontestable supremacy of their co-regency.

As explained above (pp. 2 and 4-6), there is no justification for that Italian institute to relinquish its native language in the official publication of its works, and to deface ancient texts by chalking them and to publish worthless photos of chalked texts as practically the sole photographic documentation in an edition of texts.

On the one hand, as mentioned above (p. 58), ʾrydy cannot be the ancient name of al-ʿAqûl because of the information of Ir 48/3. On the other hand, the hunting parties were held at two different places called ʾrydy and dnm which must have been contiguous or very close to each other since their names are mentioned much more frequently together than separately. There is, thus, no objection from the viewpoint of the location of the two hunting grounds against the contemporaneity of the al-ʿAqûl settlements and the reported huntings. However, it must be pointed out that there is no clue published so far suggesting that the ruined buildings studied by the Italian expedition date of the mukarribs' period.

The texts never specify what animals were hunted.

Judging from the petty number of persons who signed up on the rocks as participants, each party lacked of magnitude. No detail in the texts suggests that those hunts might have been made at any special occasion; each party looks rather like a short pleasure outing in the vicinity of a country resort to which the mukarribs occasionally retired at leisure. This seems to be confirmed by Ir 46b,58 and 68: the two maidservants of yṯʿʾmr/byn (Ir 46b) and krbʾl/wtr's nephew ʾdbʿly (Ir 58) have in common with krbʾl/wtr's young grandson ḏrḥn (Ir 68) that they had no part in any hunt but also that they stayed at that resort at least once and most probably visited at least one of the two hunting grounds.

The number of the reported participants in the hunts is deceiving. A tally of 1000 beasts beaten up by ṣbḥm (Ir 54) is difficult to understand if the beater worked alone without the help of several persons; and a tally of 1000 beasts killed by ʿmšfq (Ir 53) was possible only with a much greater help than in the preceding case because those wild animals, restless and constantly on the move would not keep still and just wait for their turn to be killed. An example of such an association for the hunt is ṣbḥm (Ir 54) with his three friends rʾshmw (Ir 61) and both

ʿmkrb and ʿmʾns (Ir 62), an association based, as one could have expected, upon clanic and professional relationship. Those four men were heads of groups consisting each of an undetermined number of individuals, and there must have been other groups of beaters beside them. In addition, if it was an honor to be invited by the mukarrib to take part in one of his hunting parties, it was another honor to confide to the rocks for posterity the record of such an invitation. The present collection of rock inscriptions illustrates that the head mukarrib yṯʿʾmr/byn made full use of his superiority to chose the best locations for his own texts and those of his followers (cf. above, pp. 47-48). There is, however, no hint in the texts that any of the mukarribs may have limited the number of the inscriptions.

Only a fanatical obsession of things ritual or religious and the habit of dreaming about the inscriptions and/or petroglyths, which have been set up by E. Anati as an ubiquitous, omnipotent principle of research, can misconstrue all those hunting parties as " a sacred hunt ... even involving the erection of a sacred monumental complex [and] representing the *mukarribs* of Saba an essential part of their investiture as sovereigns," as Garbini does (pp. 37-38; cf. above, pp. 43-45) or as " the "holy hunt" " according to de Maigret (p. 18; cf. above, pp. 59-60), who simply followed in the footsteps of Garbini whom Gnoli presents as "this authoritative scholar" (p. XII; cf. above, p. 33).

Beside the texts relating to the hunting parties, a dozen of short inscriptions (cf. above, p. 84) characterized by a more cursive lettering were engraved by passing-by travelers or locals who took advantge of the rocky panels and the presence of other texts to engrave their own messages.

The true edition of the texts remains at present impossible because of the unscientific photographic documentation and the grossly defective information put at the disposal of scholars by the direction of the IsMEO mission. Furthermore, the publication of the texts by both Garbini and al-Iryânî imposes an extra burden by their imaginative, irrealistic translations and interpretations.

It is difficult for the reader who studied the book and became aware of the lacunae, the distorted information, and the valueless photos of chalked texts not to be baffled by Gnoli's statement on

> "the first scientific work of vast commitment to be submitted to the scrutiny of scholars, presenting ample and detailed information on at least a part of the Institute's work in the Yemen Arab Republic [1]. Therefore this publication is of great importance to the Institute and especially to myself" (p. IX; my underlining).

The author did manifestly not study the manuscript before writing his eulogy which, although seemingly describing IsMEO's commendable goals, is out of touch with the publication itself. It is the ultimate of IsMEO's goals that is described by Gnoli at the

very end of the foreword of the book in these words:

" international scientific collaboration will indeed profit from the Italian work and gain from it a lasting advantage and a constant stimulus " (p. XIII).

May this public assurance

" already expressed in a conference at San'ā' on 30 October, 1985" (cf. l.c.; and also footnote 1 of p. XI) not remain sterile any longer but in the present case take the concrete form of gathering and publishing the badly needed basic material, viz. closeup photos of the texts as they are on the rock and their museographic description! Then, but only then, true research may begin in earnest on solid ground of ascertained palaeography and texts.

Concordances of the inscriptions

First concordance

Inventory numbers	Editors' sigla				
Y.85.AQ/1	Ir 61	Y.85.AQ/14	Ir 56	Y.85.AQ/27	Ir 45
/2	51	/15	59	Y.85.GP/1	66
/3	52	/16	42	/2	Ga 99
/4	50	/17	43	/3	Ir 67
/5	65	/18	54	/4	Ga 100
/6	46	/19	44	/5	101
/7	41	/20	53	/6	102
/8	49	/21	63	/7	Ir 68
/9	Ga 95	/22	Ga 96	Y.85.Y/1	47
/10	Ir 62	/23	97	/2	Ga 103
/11	57	/24	Ir 55	/3	Ir 48
/12	58	/25	Ga 98	/4	Ga 104
/13	60	/26	Ir 64	/5	105

Second concordance

Editors' sigla	Inventory numbers	Garbini	al-Iryānī (Ar pages)	Plates	My reedition
Ir 41	Y.85.AQ/7	p. 26	p. 55	pl. 41	p. 27
42	/16	30	57	45	28
43	/17	30	57	45	78
44	/19	31	59	(45),46	51
45	/27	34	59	48c	80
46a,b	/6	25	60	40b,41	79
47	Y.85.Y/1	37	61	52	28
48	/3	38	62	53b,54a,b	25
49	Y.85.AQ/8	27	63	41	31
50	/4	24	64	39b	70
51	/2	23	65	39b	75
52a,b	/3	24	66	39b	76
53	/20	32	67	46	77

Editors' numbers	Inventory numbers	Garbini	al-Iryânî (Ar pages)	Plates	My reedition
Ir 54	Y.85.AQ/18	p. 31	p. 67	pl. 46	p. 73
55a,b	/24	33	68	47c	52
56a,b	/14	30	69	43	52
57	/11	29	69	41	71
58	/12	29	70	41	71
59	/15	30	71	44	52
60	/13	29	72	42	53
61	/1	22	73	39a	73
62	/10	28	74	41	73
63	/21	32	75	46,47a	18
64	/26	34	75	48b	81
65	/5	25	—	40a	84
66a,b	Y.85.GF/1	35	—	49a	85
67a,b	/3	35	—	49b	86
68	/7	36	—	51b	18
Ga 95	Y.85.AQ/9	27	—	41	87
96	/22	32	—	—	—
97a,b	/23	33	—	47b	82
98	/25	33	—	48a	89
99	Y.85.GF/2	35	—	49b	90
100	/4	36	—	50a	90
101	/5	36	—	50b	91
102	/6	36	—	51a	91
103	Y.85.Y/2	38	—	53a	= Ja 2971F;p.23
104	/4	40	—	55a }	
105	/5	40	—	55b }	= Ja 3207;p.26
—	—	—	—	—	Ja 3206; p.25
—	—	33	—	47c,48a	Ja 3208; p.89

For all references to each individual inscription, cf. above, the description of the plates (pp. 8-9).

Word list

ʾ

ʾb (pers.s.), "hw: Ir 55b/3
ʾbkrb (pers.n.) Ir 59/1
ʾdbʿly (pers.n.) Ir 58/1
ʾdnt (place n.) Ir 48/1
ʾlmqh (div.n.) Ir 48/1,(4)
ʾlt (fem.plur.dem.pron.) Ir 43/1,44/1,45/1,50/1,51/1,52 b/1,53/1,55 b/1, (56 b/2),57/1,59/2,3,60/1,64/1; (Ga 97 b/1)
ʾmt (fem.pers.n.) "hw Ir 43/3; plur.: ʾmh:(Ir 46/b/1-2)
ʾrh (s.) b"hw: Ir 48/4
ʾrydy (place n.) Ir (41/2), 42/3,43/3,45/2,(46 b/4),49/2,50/5,(51/4-5),(52b/3-4),53/3, 54/2,55b/2,57/2,58/3; (Ga 97b/2-3); b": Ir 48/3

b

b (modal prep.) cf. ʿm,ʿly, ʾrh
(local prep.) cf. ʾrydy
(invoc.prep.) Ir 48/4;cf. ʿttr
bʾr (s.) Ir 48/3
byn (2nd pers.n.) cf.ytʿʾmr
byt (s.) Ir 43/2
bn (pers.s.) + clan n.: Ir 51/2,(52b/3),53/2
bny (v.) Ja 3206
brhmw (pers.n.) Ir 51/1, 52b/2,53/2
btʿ (gent.) "nhn Ir 66b/2

g

gdnm (clan n.) Ir 51/2,52 b/3,53/2
ghrt (pers.n.) Ga 95
gḥmt (fem.pers.n.) Ir 43/1
gnʾ (s.) Ir 48/2
(v.) cf. imperf. ygnʾ
gfśʾl (pers.n.) (Ir 67b/1)

d

]d (?) Ir 52a
dnm (place n.) Ir 41/2,43 /3,44/2,45/2,46b/4,49 /2,50/4-5,51/4,52b/3, 53/2,55b/2,57/2,59/3; [Ga 97b/4]

d

d (masc.sing.rel.pron.)cf. mdbḥm, mrš, ʿbn
dʾbm (pers.n.) Ir 67b/1
dʾnšmm (place n.) Ir 48/4
dmryfʿ (pers.n.) Ir 45/1
dmrkrb (pers.n.) Ir 60/1; Ga 102
dmrʿly (pers.n.) Ir 49/1, (56b/1), 57/1,63/2
dr[(pers.n.) Ir 64/1
drḥ (2nd pers.n.)cf.ydʿʾl
drḥn (pers.n.) Ir 68/2
dt (fem.sing.dem.pron.) Ir 43/2
dtbʿdnm (div.) [Ir 48/4]
dtḥmym (div.) (Ir 48/4)

h

-h (fem.sing.pers.pron.)cf. ḥqm, ygnʾ.
-hw (masc.sing.pers.pron.) cf. ʾb,ʾmt,ʾrh,hlf,]mḥt
hlkm (pers.n.) Ir 44/1,50 /3, 57/1,58/1
hll (v.) Ga 95
-hn (fem.plur.pers.pron.) cf. mrʾ
hnbṭ (v.;nbṭ) Ir 48/3
ḥqm (v.;qm) "h: Ir 48/2
ḥqny (v.;qny) Ir 48/1

w

wšʾbt (fem.pers.n.) Ir 46 b/1
wtr (2nd pers.n.) cf.krbʾl

ḥ

ḥlḥlm (pers.n.) Ir 66 b/1
ḥfry (place n.) Ir 48/2

h

ḫlf (s.) l"hw: Ir 48/2

ẓ

ẓwrʾmr (pers.n.) Ir 66a
ẓwrkrb (pers.n.) Ir 44/1
ẓwrʿly (pers.n.) Ir 67a

y

ygnʾ (v.;gnʾ) "nh: Ir 48/2
ydʿʾl (pers.n.) Ir 48/1,2, Ja 3207; "/drḥ: Ja 2971F
ydḥ (pers.n.) Ja 3208
yhrʿd (clan n.) Ir 65/2
ywm (conj.) Ir 41/2-3,47/2

yl²y (gent.) "n: Ir 58/1
ym'ṯt (pers.n.) Ga 99
ynrn (clan n.) Ir 66a
yqbrf' (pers.n.) Ga 100/1-2
yqhmlk (pers.n.) Ir 55b/1, 68/3;Ga 101
yqḥ (v.;qḥ) Ir 67b/2
yṯ'²mr (pers.n.) Ir 43/2,48/1,59/2,60/2; "/bym: Ir 41/1,42/1,44/2,45/2-3,(46b/2),47/1,55a,56a,(60/2),(64/1-2); (Ga 97b/1-2)

k

kbr (pers.n.) Ja 3207
khln (pers.n.) Ir 46a
klbm (pers.n.) Ir 66b/1
kn (v.;knn) Ir 68/1
krb²l (pers.n.) Ir 50/4,54/2,55B/3,(56b/2); "/wtr:Ir 49/1,51/3,52b/4,53/3-4,55b/2,57/1-2,58/2,59/3,60/3
krbm (pers.n.) Ga 95
krw (s.) Ir 41/3,47/2

l

l (dative prep.) cf. hlf
lḥy (pers.n.) Ga 98
lḥy'ṯt (pers.n.) Ir 63/1
lḥyṯt (pers.n.) Ir 65/1

m

mdbḥm (clan n.) d": Ir 61/3,5
mwd (pers.n.) Ir 48/1; Ja 3207
mḥt (?) "hw: Ir 48/3
mkrb (pers.n.) Ir 41/2,42/2,47/1,49/1-2
ml²km (pers.n.) Ir 62
mnql (s.) (Ir 48/3)
m'dkrb (pers.n.) Ir 56b/1
mqtw (pers.n.) Ir 61/2
mr² (pers.n.) "hn: Ir 46 b/3
mrṣ (clan n.) d": Ga 95

n

nbṯ (v.),cf. 4th form hnbṯ
-nhn (emph. dual end.),cf. bt','lm

s

sb² (place n.) Ir 41/2,42/3,47/1,49/2
syḫn (family n.) Ga 101
smh²mr (pers.n.) Ir 50/2
smh'ly (pers.n.) Ir 41/1, 42/2,47/1,59/1
smhrym (pers.n.) Ir 68/1

'

'bn (clan n.) d": Ir 62
'd (local prep.) Ir 48/4
'lhn (family n.) Ir 67a
'ly (modal prep.) b": Ir 48/2
'lm (s.) "nhn:(Ir 48/1)
'm (prep.) Ir 44/2,45/1,54/2,56b/2,57/1,58/2,59/2,3; b": Ir 46b/3,50/3; 51/2,52b/4,53/3,55b/3, 60/2,3,[64/1],[Ga 97 b/1]
'm²mr (pers.n.) Ja 3206
'm²ns (pers.n.) Ir 62
'mkrb (pers.n.) Ir 62
'mṣfq (pers.n.) (Ir 52a, b/1-2),53/1
]'fl (pers.n.) Ga 97a
'ṯtr (div.) Ir 41/3,47/2; b": Ir 48/4

ġ

ġlftn (pers.n.) Ir 65/1-2

f

fq (cf.;fwq) Ga 98
fqḍ (pers.s.) Ir 54/1,61/4, 62

ṣ

ṣ[(?) Ir 64/2
ṣbḫm (pers.n.) Ir 54/1,61/4
ṣd (s.; = ṣyd) Ir 41/3
(v.; = ṣyd) Ir 41/2,3,42 /3,47/2,49/2
ṣyd (s.; = ṣd) Ir 47/2
(v.; = ṣd) Ir [52b/1], 53/1,55b/1

ḍ

ḍr'm (place n.) Ja 3207

q

qḥ (v.;qwḥ) cf.imperf. yqḥ
qyf (s.) Ir 47/1-2
qm (v.;qwm) cf. 4th form hqm
qny (v.) cf. 4th form hqny
qf (v.;qyf) Ir 47/1

r

r²shmw (pers.n.) Ir 61/1
rwym (place n.) Ir 48/3
ryd (v.) Ir 48/3
rnt (clan n.) Ga 97a
.r'. (fem.pers.n.) Ir 46b/1

š

šnʾ (v.) Ir 44/1,45/1,50/1,51/1,(52b/1),53/1,54/1,55b/1,56b/2,57/1,59/2,60/1,64/1; [Ga 97b/1]; (fem.) "t: Ir 43/1.

Abbreviations

ARGAY : J. Schmidt et al., Archäologische Berichte aus dem Yemen, Mainz am Rhein, I - IV (1982 - 1988).

Ar : Arabic.

CIASA : Corpus des inscriptions et antiquités sud-arabes, Paris, I - II (1977 and 1986).

FrLAL : G.W. Freytag, Lexicon arabico-latinum, Halles, I-IV (1830-1837).

Ga : G. Garbini. Ir : M.A. al-Iryânî. Ja : A. Jamme.

JaCIASA : A. Jamme, "Corpus des inscriptions et antiquités sud-arabes," in Orientalia, 48 (1979), pp. 260-284.

JaMAR : A. Jamme, Miscellanées d'ancient arabe, Washington, I (1971), II (1971), VII (1974), X (1979), XI (1980), XIII (1982), XIV (1985), XV (1987), XVI (1988).

JaSTMB : A. Jamme, Sabaean Inscriptions from Maḥram Bilqîs (Mârib), Baltimore, 1962.

Mā : North-Arabic.

PiPESA : J. Pirenne, Paléographie des inscriptions sud-arabes, Brussels, 1956.

RÉS : Répertoire d'épigraphie sémitique, Paris, V - VIII (1928 - 1968).

RyCRA : J. Ryckmans, "La chasse rituelle dans l'Arabie du Sud ancienne," in al-Bahit, Festschrift J. Henninger, St. Augustin, 1976, pp.295-308.

Sā : South-Arabic.

SDBS : A.F.L. Beeston - M.A. Ghul - W.W. Müller - J. Ryckmans, Sabaic Dictionary - Dictionnaire sabéen - al-muʿjam as-sabaʾī, Louvain-la-Neuve, 1982.

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